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ORIGINAL ARTICLES.

THE PREVALENCE OF ALBUMINURIA IN PERSONS APPARENTLY HEALTHY.

Read in the Section of Practice of Medicine and Physiology, at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D. C., May 5-8, 1891.

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I read a paper before the American Medical Association at its Forty-first Annual Meeting, held in Nashville, Tenn., May 20th, 1890, on the subject of "Functional Albuminuria, or Albuminuria in Persons Apparently Healthy," in which I reported a case which had been under my observation for five years. On March 5th of this year, 1891, I made a careful urinalysis of this person's twenty-four-hours' urine, examining separately each specimen as voided, and then mixing them together and again examining a sample of the whole quantity. The tests used were cold nitric acid,—contact method,—and boiling, then adding a drop or two of nitric acid and again boiling. The following are the results, viz.:

	COLOR.	RE-ACTION	SPEC. GRAV.	ALBUMEN.
Urine passed at 7 A.M.	Dark straw	Acid	1.030	No albumen.
" " 11 A.M.	Straw.	"	1.020	Faint trace.
" " 3:15 P.M.	"	"	1.028	Faint trace.
" " 7 P.M.	"	"	1.020	Faint trace.
" " 9 P.M.	Dark Straw	"	1.028	Small quantity
" " 10 P.M.	Straw.	"	1.020	A trace.

Whole amount of twenty-four hours' urine, 32 ounces; reaction, acid; color, dark straw; specific gravity, 1.025; albumen, a faint trace. Without the addition of the 9 P.M. urine, albumen was not discoverable. The 9 P.M. urine was the only sample which gave any deposit of sediment after standing twenty-four hours. A microscopical examination of this sediment resulted as follows, viz.:

1. *Casts.* Only an occasional hyaline cast.
2. *Crystals.* Calcium oxalate crystals present in great numbers.
3. *Epithelium.* Pavement and small round bladder epithelial cells present in small numbers.

Another examination of his urine was made on March 21st, 1891, with about the same results, except that less reaction was obtained than on any former examination. The night urine contained no albumen, and the urine passed two hours after his mid-day lunch contained the most albumen; after standing twenty-four hours it constituted one-twentieth of the urine in the test tube. The specific gravity of the twenty-four hours' urine was 1.028, reaction acid, color dark straw. During the six years he has been under my observation his twenty-four hours' urine has never exceeded 40 ounces in quantity and the specific gravity has never been lower than 1.020. His early morning urine has never contained albumen, although it has invariably been present in small quantity at some hour later in the day. He has always been, and still is in the best of health seemingly. He has not had a day's illness in ten years. He has not had rheumatism, gout, lead poisoning, syphilis, nephritis, dyspepsia or dropsy. No member of his family has had any of the above diseases or any history of Bright's disease. In his case there is no increase of vascular tension, no cardiac hypertrophy, palpitation or dyspnœa, and no retinal symptoms. He is 36 years of age, temperate in all his habits, happily married, prosperous in his business, resides in a suburban home where he spends his time after business hours. This case of albuminuria, which I present to you, is not a hypothetical,—a supposititious case, such as our learned brethren of the bar are accustomed to fire at the unfortunate disciples of Æsculapius, when they get them at short range upon the witness stand, but he is a living being, like ourselves, who has been under my observation for the past six years. His case is not an exceptional one, but a typical one, and has been observed, studied and recorded the world over. Pavy designates this condition as "cyclical albuminuria." Senator, who fully endorses Pavy's observations, prefers the term "paroxysmal," while other equally eminent observers believe that "intermittent" is the best name for it. I shall not consume any time in the discussion of the merits of these particular names, but shall speak of this form of albuminuria, and the other forms which may occur in persons apparently healthy, as "functional albuminuria."

Now what is the clinical significance of the albuminuria in a case like this? If this man had not made application for life insurance six years

ago, no one, up to this date, would have known, or even suspected that anything was the matter with him. For all practical purposes he was and is a perfect specimen of manhood. During these six years he has become the father of three healthy children, has amassed a fortune in his business, is a public spirited citizen, and prominent in all benevolent and church work.

During the past fifty years albuminuria has been regarded as the one infallible symptom—if not synomyn—of Bright's disease. As a consequence, the patient in whose urine albumen was found, was regarded as a doomed man, as Bright's disease was believed to be necessarily fatal. After a time, however, it was found that albuminuria was frequently present in other diseases, as for example, pregnancy, the puerperal state, zymotic and other febrile diseases, lead poisoning, etc. These discoveries modified its significance somewhat, but did not materially reduce the gravity of its presence in other cases. Then, such a veteran authority as Prof. Geo. Johnson asserted that albumen, to a large amount, may be constantly present in the urine of men, women and children who are apparently in good health, and he even went so far as to say that "it has been proven by abundant experience that the mere fact of albumen, even in large amount, filtering through the walls of the malpighian capillaries does not seriously, if at all, interfere with the proper excretory function of the convoluted tubes."

Then, Finlayson, Mahomed, Fraser, and other eminent authors,[1] demonstrated that structural disease of the kidneys, or Bright's disease may actually be existant without albuminuria, and Prof. Fraser informs the profession that "albumen is only one symptom" and that all forms of Bright's disease are accompanied by other symptoms.

Then, Prof. Grainger Stewart makes the remarkable statement "that cases of Bright's disease do not account for one-half of the cases of albuminuria met with in practice," and Dr. Saundby, in his recent work on Bright's disease, makes this astounding utterance: "The practical outcome of the study of the incidence of albuminuria in the sick and the healthy should be to extinguish altogether the pernicious doctrine that albuminuria means organic disease of the kidneys, a doctrine less harmful than the equally fallacious one that organic disease of the kidneys is a rapidly fatal disease." And then the *British Medical Journal*, in a leading editorial makes the broad announcement that "albuminous urine is no longer the equivalent expression for Bright's disease," and says "the attention of the profession, during the last few years, has been called to the fact, by repeated observations, that albumen may be found in the urine of persons who do not exhibit any of the general symptoms of Bright's disease, nor even any evidence of any deterioration of health." Finally, Prof. Moxon, after relating the various causes which will produce temporary albuminuria, says: "Albuminuria becomes in relation to disorders of the renal system what neuralgia is to disorders of the nervous system, or what dyspnœa is in disorders of the respiratory system—a name of a symptom—which is on the very outskirts of an intimate knowledge of any case, and thus albuminuria, as Dr. Bright knew it, becomes by-gone and historic."

These utterances from such authoritative sources are sufficient to warrant the opinion that the significance of albuminuria, as first announced, was overestimated; nevertheless, its grave significance is so thoroughly ingrained in the profession that we can scarcely admit of any other condition than a pathological one as being compatible with its presence.

Prevalence of Albuminuria.—Until a few years since, it would have been considered rank heresy for any person to have suggested that albuminuria was compatible with any other condition than that of organic renal disease, and a physician would have been looked upon as somewhat unbalanced in his mind who would have asserted that albuminuria would occasionally be found in persons apparently healthy, but now the medical press, in both Europe and America, claim that it has been found in a large percentage of people otherwise healthy; and this claim has been supported by so many eminent physicians the world over, that there seems to be no doubt of its correctness.

In our country Shepherd found albumen in the urine of 2 per cent. of 35,471 persons examined. Washburn, of Milwaukee, found it in 5.91 of 338 examinations of healthy persons for Life Assurance. Munn found it in 11 per cent. of a large number of individuals "who considered themselves perfectly healthy." Edes states that the presence of albumen in minute traces, or something that gives a similar reaction with the most delicate re-agents, is much more common than its absence, and the amount detectable by heat and nitric acid may be approximately stated as from 10 to 20 per cent. of persons examined.

In Great Britain and in Europe larger percentages have been found. Grainger Stewart, in a report to the Royal Society of Edinburgh, stated that he found albumen present in 31 per cent. of 407 urines from healthy individuals and he concluded his report by saying "that albuminuria is much more common among presumably healthy people than was formerly supposed, tests having demonstrated its presence in nearly one-third of the population."

Dr. Turner, Medical Officer of Essex County

[1] Dr. Purdy, of Chicago, in 1885, collected from Guy's and other hospital reports, 259 cases of chronic Bright's disease in which albumen was present in only 68 cases, leaving 191 cases, nearly 74 per cent., in which albumen was absent.

Insane Asylum, England, found albumen in the urine of 40.2 per cent. of 200 male inmates, and in another Insane Asylum, Kleudgen found albumen in 43 per cent. of the healthy nurses. Stirling found it in 44 per cent. of 461 healthy adults examined by him; Capitan found it in 45 per cent. and in children 89 per cent.; De la Celle-Chateauberg says it may be found at times in the urine of 76 per cent. to 100 per cent. of healthy persons, both young adults and children; Posner asserts that he has proven the existence of albumen in all normal urine, and his experiments have received the stamp of approval from such men as Senator, Duden, Leube and V. Noorden. Washburn, of Milwaukee, in *The Medical News* April 5th, 1890, says that in order to verify the claim of Posner, he tested with picric acid by the contact method, and also with the phenic-acetic test, and with a solution of citric acid of a specific gravity of 1.008 by the same method, samples of urine from fifty persons in perfect health, all living in good sanitary surroundings, and "in every instance, at the line of contact of the two fluids, the characteristic cloud appeared with more or less distinctness indicating the presence of an albuminoid."

Prof. Senator, of Berlin, in his recent work entitled "Albuminuria in its Physiological and Clinical Relations" Berlin, 1890, says "the more complete methods of investigation and the application of delicate tests have had as a result the discovery that albumen is frequently found in the urine of men who exhibit neither objective nor subjective symptoms of disturbances of health, and who after a long continued observation, appear to be perfectly healthy." From the reports of a number of the best authors, he says: "we are warranted in believing that albumen is found in the urine without any other discoverable pathological changes, in a surprising number of cases. Forty-one out of every one hundred healthy, strong men, especially soldiers, under ordinary circumstances have albuminuria." Senator holds that we may regard the appearance of albumen in the urine of healthy men as the "Physiological albuminuria, just as there exists a physiological glycosuria, oxaluria and indigouria."

Sir Wm. Roberts says it has been both affirmed and denied, on high authority, that traces of serum-albumen exist in normal urine. "I have satisfied myself" he says, "that concentrated urines from persons in undoubted health are comparatively rarely free from traces of albumen, detectable by direct testing. In ordinary processes of testing urine, these traces are naturally overlooked, but they certainly exist very frequently, and their existence shows how nearly on the verge of a sensible albuminuria healthy people are."

The Proteids of the Urine.—It is somewhat remarkable that the recorded observations in our country show a very low percentage of albuminuria in persons apparently healthy when compared with those of our British and Continental cousins. I asked Dr. Victor C. Vaughan, Prof. of Chemistry in the University of Michigan, why it was that physicians in Great Britain and Europe, men of undoubted ability and world wide fame, were finding albumen in the urine of 20, 30, 50, 70 and 100 per cent. of healthy persons examined by them, when the highest per cent. reported in our country was but from 10 to 20 per cent? In reply to my inquiry he wrote: "I will agree to the statement that *proteids* are frequently found in the urine of the healthy, or those who so far as they themselves or any one else may know, are healthy", and he called my attention to an article on "The Proteids of the Urine, with a comparison of the Tests for Albumen" by F. G. Novy, M S., Instructor in Physiological Chemistry, University of Mich. (*Medical News*, Vol 53, 1888), and said "you will see from this article, to which I have referred you, that with some of the proposed tests you can find albumen in the urine of almost any one." In this article Mr. Novy calls attention to the fact that serum-albumen is the only proteid which the average physician expects to find in the urine. Whereas, globulin, hemi-albumose (Bence Jones albumen) and peptone, are also frequently present. In the ordinary methods for testing for albumen, all of these proteids but peptone may be readily mistaken one for another, and, indeed, he says, "this has been rather the rule than the exception." Serum-albumen is generally considered to be indicative of structural disease of the kidneys, but the other proteids have no such grave significance, nevertheless serum-albumen is usually accompanied by larger or smaller quantities of globulin, and in some instances, even peptones may be present, "our ordinary tests for serum-albumen are also responded to by globulin and, hence, when applied to an albuminous urine, they indicate, as a rule, a greater amount of serum albumen, than that actually present."

Globulin usually accompanies serum-albumen and may be present in almost all varieties of albuminuria, though in variable quantity. According to Hammaisten, it constitutes from 8.13 to 60.24 per cent. of the total proteids in albuminous urine. The more recent investigations of Maguire indicate that globulin can exist in the urine by itself, even without serum-albumen being present, and he reports three cases of cyclic or functional albuminuria, and one of puerperal albuminuria, where he found the proteid of the urine to consist solely of globulin. Novy also mentions one case of functional albuminuria which came under his observation where globulin alone was found in the urine. These reports accord with the statement I made in my paper on "Functional Albuminuria" read before the American Medical Association May 22d, 1890, viz.:

"The present line of investigation is pointing strongly toward globulin as the form of albumen which is likely to be found in the various forms of functional albuminuria when chemistry will furnish us with simpler methods and more reliable reagents for its discovery."

ELECTRICITY AS A THERAPEUTIC AGENT. WHAT IS NEEDED TO DETERMINE ITS MERITS?

Read in the Section of Practice of Medicine and Physiology, at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D. C., May, 1891.

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There are few subjects toward which the mind of the medical practitioner is more in need of proper adjustment than that of the use of electricity as a therapeutic agent. It is now more than a century since a professor of anatomy in the University of Bologna announced to the profession—that is daily puzzling its brains over the various ills that flesh is heir to—that he had discovered, in the association of dissimilar metals with the legs of decapitated frogs, a force was generated capable of acting as a substitute for nerve impulse in causing muscular contraction.

Electro-physiological inquiry received a new impulse from this discovery of the effects of a continuous or galvanic current on living tissue, and from that time on there has been an unceasing activity displayed by many of the best minds in the profession, in adding to our knowledge of the power of electricity in modifying and controlling the vital processes going on in animal structure, until medical and scientific literature is burdened with the rich fruit of their researches.

Many of the discoveries in electro-physiology suggest valuable additions to our therapeutics, and while there have not been wanting examples of able men in the profession, like Remak, Duchenne, von Zeimssen, Erb and De Watteville, who have used their best energies to gain for electro-therapeutics the recognition which its importance deserves, its power as a curative agent is as yet but imperfectly understood by the vast majority of physicians. The reason for this is readily explained. The work of the physiologist, though indispensable as antecedent to rational therapeutics, is often in too crude a state to be readily made available by the busy practitioner. A hiatus exists between the discoveries of pure science and their practical application to the requirements of everyday life. Men are rare who have either the time, the opportunity or the ability to acquire such experience in both fields of research as to adapt the discoveries of the one to the needs of the other. Electric physics, physiology and pathology, each are so comprehensible in their scope that, with the best facilities, a lifetime is spent in acquiring a knowledge of what is known of each. It is no wonder, then, that, during the past century, in which so much has been revealed concerning the mysterious working, both on the organic and inorganic world, of that strange force we call electricity, so little practical use has been made of it by the physician, and that it has, in common with many other discoveries, in the twilight of their early dawn, been seized upon by the impostor and charlatan as suitable instruments with which to prey upon the minds and pockets of a credulous and expectant world; and when, in addition to this, we recall the fact that there has existed among those who have attempted to direct our minds toward the therapeutical application of electricity, the greatest diversity of opinion as to the form of currents that should be used, and the methods for applying them, and that no attempt has been made at uniformity even in the instruments employed for this purpose, it is not surprising that electricity as a therapeutic agent has been looked upon as of rather doubtful efficiency by the majority of physicians. But, fortunately, while these causes have operated to bring electricity into disrepute in the minds of conscientious practitioners of medicine, the rapid strides it has made in other fields of usefulness have raised it in importance and esteem, so that its power and efficiency must be everywhere recognized by the man, be he medical or lay, whose daily life is so dependent on its labor. It now requires but little argument to convince any man that electricity may probably effect some change in the disordered state of his bodily economy, who, every hour of the day, is depending upon this selfsame force to aid him in the minutest details of his daily life. Nor does the *physician* now entertain that same contempt which once he might have done for an agency which, in these latter days, lights up his house, washes his linen, stitches his clothes, prints his newspapers, cooks his meals, transports him to his office, and brings him into communication with his patients, far or near, while sitting at his desk. So insidiously have the commercial uses of electricity crept in upon us, and so indispensable have they become already to our comfort, that were we, as a people, thrown back on the resources of even a decade ago, we would feel somewhat the same uneasiness and discomfort that Rip Van Winkle expressed on waking from his long sleep. While the profession at large has given but little or no attention to the therapeutic uses of electricity, there has been a skirmish line in advance of the main body, that has not been idle, and along this line there have not been wanting bold and self-reliant spirits who, with the directness of aim and the clearness of observation of the sharpshooter, and with a genius for experiment and fertility for invention seldom excelled, have done efficient work in making in-

roads on diseases that have not yielded to other modes of treatment; or, if so, with far less readiness.

We do not question that claims have been made for electro-therapeutics that are fallacious and unsound, but, granting this, the work of the last fifteen or twenty years leaves much that can withstand severest criticism, and will compare favorably with the results obtained by any other method for treatment of disease, that receives the unqualified approval of the profession. The work of such men as Erb, De Watteville, Apostoli, Keith, Beard, cannot be turned aside with a sneer; while the universal testimony of the neurologists that this agent is indispensable to the successful treatment of a long range of diseases coming under their management, challenges our acceptance and respect.

The therapeutic effects of electricity, as far as they have been determined by physiological and clinical observations, may be classified as *stimulant*, or *electro-tonic*, *sedative*, *electrolytic* and *cataphoric*. To these should be added the *cauterizing* action, since the galvano-cautery has a range of therapeutical application peculiar to itself.

STIMULANT ACTION.

The *stimulant and electro-tonic* influence of certain forms of electrical application is readily demonstrable, and is applicable to a great variety of conditions of bodily derangement. Where muscular tissue lacks tone, whether it be voluntary or involuntary, striped or unstriped. it can be readily subjected to the stimulating influence of the electric current, and in a variety of ways. For this purpose we can employ the static machine, the cathode of a continuous current, or the induced current completely interrupted or alternating, and, by the selection of one or the other of these forms, according as it seems best adapted to the condition, the functional vigor can be aroused in an organ in which such muscular tissue forms an essential part. And how universal are such tissues throughout the body! Thus the circulation in any part may be quickened, passive congestions overcome, congested organs relieved, and the evil results of malnutrition from excess or lack of blood in a part, or from feeble efforts at elimination, may be corrected by this action of the electric current. Functional disorders due to failure in vaso-motor control are especially benefited by such applications.

The good effects of such currents are daily demonstrated in the clinic by increased growth in paralyzed limbs, relief of pain, improved action of the viscera, and an increased sense of well-being manifested by the patient's manner and action. A torpid liver, a congested spleen, constipated bowels due to lack of secretion or feeble peristalsis, a congested or subinvoluted uterus, chronic congestion of the ovaries, exhausted nerve centres with attendant hyperæmia or anæmia, producing those perplexing states of neurasthenia or spinal irritation, are all conditions that can be helped by electric stimulus. And what have we to look to in our materia medica that will accomplish like good results with less damage to the organism as a whole? What can you apply or introduce that will reach the offending spot with as much precision, or do less damage by its presence while it gives the needed help? It certainly adds energy where energy is wanting, and when its work is done it leaves no *dèbris* behind to clog the further working of the part.

SEDATIVE ACTION.

As a temporary *sedative*, both the induced current and the anode of a mild continuous current are of service. They calm the neuralgic pains of many functional and organic disorders; they relieve headaches due to congestion or migraine; they allay the pains of muscular rheumatism, of pleurodynia and painful menstruation, and the lightning-like pains, so common in locomotor ataxia, are often more promptly and permanently controlled by this than by any other means. The manner of action through which this effect is brought about by the electric current is open to speculation. It may be in one of a variety of ways: by subduing a congestion which is the cause of the pain; by stimulating absorption or elimination of some deleterious effete substance; possibly by hastening the decomposition of an irritating product which is formed as the result of imperfect or misplaced metabolism; or the purely physical influence of vibrating movement induced in the nerve structure may account for the soothing effect. Whatever it may be, the calmative influence of these methods of electric application is daily exhibited in a great variety of painful affections, and sufferers of all ages and temperaments bear grateful witness to its efficiency in affording them relief.

ELECTROLYTIC ACTION.

The electrolytic action of the continuous current—that is, the power it has to bring about chemical decomposition and change the atomic arrangement in the composition of compound substances, is credited with very important therapeutic effects.

Electrolysis, by freeing the acid constituents of the tissues at the anode, and the alkaline at the cathode, is a convenient means for producing styptic or caustic action, which can be readily applied and accurately limited, and has of itself a wide range of application in the diseased body. But if, as is claimed, it can be proved by actual experiment or from clinical data, that all tissues lying between the rheophores of a continuous current are directly modified in nutrition and vital action by its influence, according to the quantity or intensity of the current, who is there so rash as to attempt, in this the infancy of

the rational use of electricity as a therapeutic agent, to mark out the limitations of its powers? No matter where, upon or within the body, disease has begun its havoc, the continuous electric current may be made to traverse the part and work such change as comes within the province of its powers. What these powers are over living tissues the physiologist and clinician must determine, but may it not be, with some reason, anticipated that normal tissue will be found to be stimulated to more vigorous action and resistance by its aid, while abnormal processes are checked, and substances foreign to the part, as effete matter or germ colonies, will be found unable to withstand the additional energy of this opposing force. If such change can be proved to be wrought by this mysterious agent in the secret laboratory, where cells and blood carry on their incessant interchange in harmony or discord, then no one will deny that in the continuous current we have a therapeutic agent which, for many disorders within the realm of both medicine and surgery, surpasses all other means at hand, and testimony in medical literature is accumulating in such abundance and from such high sources, as the result of accurate methods and critical observation, to the effect that herein we have a resource which will *cure* where hitherto we have only been able to *destroy* the part affected, that it challenges investigation, if not adoption, by every member of the profession.

CATAPHORIC ACTION.

The cataphoric action of the continuous current gives promise of aiding us materially in many ways. The introduction of medicinal agents into the body through the skin is oftentimes desired, and any method that will accomplish this with greater certainty than those now in use will be a valuable acquisition. The discovery of lanolin as an excipient for remedies applied by inunction was a great advance. But if, by employing the anode of the continuous current, we can hasten this process of absorption, and cause medicines to penetrate the cuticle, and thus reach directly the offending tissues, it will, in many instances, be of great service. This power to increase or reserve osmosis which the continuous current is said to possess will, if true, have abundant duties assigned to it in the removal of dropsical effusions from cellular tissues, joints and cavities. The contributions of Munk, von Bruns, Corning, Peterson and others, have furnished us with much interesting information on what they claim is the cataphoric action of electricity in carrying remedies and anæsthetics through the skin, and it has even been suggested by a well known and highly esteemed surgeon, in a recent article, "that the principle of the process that causes absorption or diminution of the bulk of an uterine fibroid treated in this manner (*i. e.*, by the constant electric current), must be looked for in some form of electrical osmosis."

CAUTERY ACTION.

The advantages offered by the galvano-cautery over any other surgical procedure are chiefly found in the removal of growths of vascular or fungous character, or in arresting the progress of ulcers and malignant disease in passages of the body narrow and remote from the surface, like the nasal fossa, the Eustachian tube, the pharynx, larynx and external auditory meatus, the bladder, urethra and uterus. By this method the cautery snare, or knife, or scoop may be nicely adjusted to the necessities of the case, and when all is in readiness the heat is generated in the cauterizing tip with a definiteness of application and rage of intensity perfectly under control. In skilled hands its action is perfect and for such cases all other methods of treatment are but bungling substitutes.

Let us briefly enumerate a few of the results attained by electricity which have directly advanced the science of medicine and surgery. The physiologist, by the discovery of the laws of electro-tonus, has made it possible for us to determine the condition of a paralyzed muscle, thus greatly advancing the accuracy of diagnosis after injury or disease of brain, or cord, or nerve.

For the increase of our knowledge of *cerebral localization*, with the marvelous results in *cerebral surgery* which have followed so rapidly in its train, we are almost wholly indebted to the electrical experiments upon the brains of animals by a host of investigators. As a remedial agent it is claimed that:

It restores the action and increases the growth of paralyzed parts; it arouses vital action; it improves nerve conductivity; it contracts muscle; it hastens osmosis and absorption; it excites secretion; it quickens the circulation; it aids elimination; it allays pain; it makes hypodermic medication possible without puncturing the skin; it destroys superfluous hairs, removes warts, moles, sebaceous cysts and epitheliomas; it checks the ravages of lupus and other ulcers; it heals bed sores; it arrests keloid growths; it dissolves away cicatrices in the skin and along mucus channels forming strictures; thus, stricture of the lachrymal duct; thus, stricture of the Eustachian tube; stricture of the œsophagus; stricture of the os uteri; stricture of the rectum; stricture of the urethra, are all amenable to this treatment. "Not long ago physicians and surgeons of repute flouted the treatment of urethral strictures by electrolysis, now it is so generally and successfully practiced that scarcely any oppose it. . . . . There is no comparison between the treatment of strictures of the urethra by the ordinary methods and its treatment by electrolysis. Should the permanency of the good results prove to be, as a rule, not so great as those recorded, still the calibre of the stricture remains

enlarged for a longer space of time than after any other form of treatment." [1]

Like testimony from reliable sources as to its superiority over all other methods for the relief of strictures and the disorders consequent upon them in the other localities named, is not wanting.

It coagulates the blood in aneurisms, nævi and varicose veins.

Dr. John Duncan, surgeon to the Edinburgh Royal Infirmary declares "I have no hesitation in saying that the only justifiable method of treatment for cirsoid aneurism is electrolysis."

We all know the claims of Apostoli and his followers for the treatment of uterine fibro-myomata by electrolysis, and it needs but be said that larger experience and improved methods have but confirmed and rendered more positive his earlier view, i.e., that it is a method for treating the majority of cases superior to all others, and in this he is ably backed by Keith and numerous lesser lights. Keith's latest conclusions are thus stated in his own words:

"This treatment *almost always* relieves pain. It *almost always* brings about diminution of the tumor, sometimes rapidly. It *almost always* stops hæmorrhage, sometimes rapidly. The results are *almost always* permanent and the growth of the tumor, if it be not lessened, is stopped. The general health is *immensely improved.* By *almost always* I mean *nineteen cases* out of *every twenty.*[2]

The prostate gland, homologous in structure with the uterus and subject to like abnormal growth, has proved a perplexing problem when enlarged. Both physician and surgeon have expended upon it their best endeavors with but small reward; but some encouragement may now be held out to the sufferer and his physician, for recently it has been said, by one whose experience and opportunity for observation give weight to his words, "We have no hesitation in defining this as the only truly remedial treatment known for the hypertrophy of this gland in *restoration of its function*, operative procedure, looking, as is evident, to its greater or less destruction."

Such an array of victories claimed in the name of electricity demands of the profession proof or disproof.

But how are we to go about separating truth from error in these claims, and by what means can we determine with accuracy the capacities and limitations of this powerful force in nature for the treatment of disease? Hitherto we have, for the most part, acted like children in our dealing with the matter, for, armed with the trifling toys so often furnished for medical batteries, as heterogenous in their variety and construction as the weapons of our revolutionary forefathers in their first contests with the King's regulars, we attempted a crusade against the hidden mysteries of disease with a force, in its essence powerful, it is true, but hampered by our ignorance and the conditions under which we sought to operate it. No wonder that failure in cure followed such crude applications and that rumors of better results were met with incredulity.

We believe we are about to enter upon a more thoughtful age and are now as a profession prepared to soberly seek out and conform to those conditions which are indispensable for determining the effects of this all prevailing-force on the human body, both in health and disease.

THE FIRST REQUIREMENT.

The first requirement for a rational electro-therapeutist is that he who undertakes to practice it should be well drilled in electric physics and physiology.

In no branch of practical medicine and surgery can a knowledge of the fundamental principles of the natural sciences be safely ignored, but nowhere is such knowledge so indispensable to success as in the practice of electro-therapeutics, and yet how many of our medical colleges demand even an elementary training in physics as a condition for entrance? And how few furnish any instruction worthy the name in the regular curriculum?

How many physicians who are daily making use of electricity in their practice could explain the construction of the apparatus they are employing or the conditions it must comply with in order to generate a current? I have known a physician in high standing, a professor in a medical college, to use a galvanic battery of simple construction for a period of three months in making daily applications to a patient, and during that entire time there was no current passing through the conducting cords, he having failed to join the proper connections. And such mortifying instances, discreditable to the profession, could be related *ad nauseum.*

One who makes use of an electric battery should be as familiar with its construction and action as an oculist with his ophthalmoscope or an engineer with his engine; but how many expensive batteries, do we see lumbering up physician's offices, useless and set aside because of a corroded connection or a broken wire! It is perhaps fortunate so many accidents do happen to medical batteries, for electricity is a dangerous tool in the hands of one who knows so little of the conditions upon which its action depends.

Hand in hand with ample knowledge of electro physics should go a familiarity with the action of electric currents on the normal living tissues. The laws of electro-tonus; the effects peculiar to the anode and the cathode; the variation in resistance of different tissues, all are essential factors in the information requisite to an in-

[1] Electrolysis in the Treatment of Urethral Strictures. New Eng. Med. Monthly, Dec. 1889.
[2] W. E. Steavenson, M.D., Woods Monographs. "Use of Electricity in Surgery."
[3] Brit. Med. Jour., Feb. 14, 1891.

telligent use of electricity in therapusis. Practical demonstrations and practical experience in electro-physiology as laboratory work under competent instructors, can alone fit the student for practicing intelligently this occult science. As reasonably might the physician, or he who dignifies himself by that title, attempt a cure by dealing out unknown drugs from his medicine case as to use electricity on his patients without such antecedent preparation.

THE SECOND REQUIREMENT.

The second requirement for securing a rational scientific position for electricity among therapeutic agencies is a uniformity in the means employed for its application to the body.

We have at present no less than fifty manufacturers of electric batteries and appliances for medical use, and no two of them have adopted the same pattern or means for generating currents. On the contrary, trade enterprise has stimulated inventive ingenuity to produce diversity rather than uniformity in order that it might be made an additional argument for the sale of that particular variety of instrument. Many physicians in this, as in many other therapeutical measures, take suggestions from non-medical men as to what instruments they require and how their patients should be treated. Those members of the profession, who have from personal experience, discovered the essentials of a medical battery rather than the instrument maker, should be looked to for information as to what are the best and most suitable forms of batteries for therapeutical purposes. Some standard should be adopted for a constant current battery, an induction coil, a volt meter and a milliampèremeter.

It is rare, if any two physicians at the present time, who daily employ electricity in their practices, do it in like manner. They cannot therefore compare results since they are not assured that in the treatment of similar diseases they have subjected their patients to the same conditions. Should I in Michigan wish to test upon my patient the effects of electric treatment claimed by a fellow practitioner in New York or elsewhere to be successful, how can I know that my "McIntosh" is furnishing the same current that he used from his "Kidder," since our milliamperemeters have never been compared, and the voltage and resistances are not reported?

Recently effects have been reported in a prominent journal by an eminent electric therapeutist as having been brought about by the use of the induction current that seems to me incredible, but am I justified in denying his statements until I know what form of instrument he made use of, and have myself used the same in like manner and so determined the inaccuracy of his conclusions?

Nothing differs more than the intensity of the currents generated by induction coils; therefore, without a standard instrument, which is the same wherever used, no progress toward uniform results can be made.

Until within a few months there has seemed to be no way open for securing this much desired harmony and uniformity in electro-therapeutical appliances, but the organization of an *American Electro-Therapeutical Society* in January last gives promise of better things, and may we not hope that through the labors of this organization we will soon receive definite instructions as to the best instruments to make use of and the best methods for applying them?

In this connection let me suggest that the tendency should be toward the simplest and least expensive instruments consistent with efficiency. Many of the obstacles to the progress of electro-therapeutics have arisen from the high price, complexity and frailty of the instruments furnishing the current.

One drawback, and that perhaps the most fruitful in retarding good work, lies in the use of fluid batteries for generating both constant currents and the primary circuit for induction currents. Many of the annoyances arising from this source, such as the deterioration of the fluids, the consumption of the elements when the battery is not in use, the corrosion of connections in the circuit, can now be overcome by using a dry cell. I have for months made use of a dry cell for running induction coils and found it very satisfactory. You who have occasion to use portable batteries can readily appreciate the advantages to be derived from substituting a dry for a wet cell in all portable machines. Even for stationary work the dry cell is superior on account of its cleanliness, but in matter of durability it requires some improvement.

Nothing, in my opinion, will eventually be found so convenient for office work as the use of commercial currents.

The Edison incandescent current with its constant voltage of near 110, affords an admirable source of electric power for electrolysis, cataphoresis, inductive stimulation, and cautery purposes. By a very simple appliance for changing the voltage I have used this current in my office for more than two years, and the same method applies quite as well to the currents of equal or less voltage from other sources, whether they be alternating, as the Thomson-Huston dynamo, the storage battery, or any form of primary battery plant that has sufficient electro-motive force.

To sum up, therefore, the ideal conditions for establishing electro-therapeutics on a scientific basis, they are:

1. A *source of electric power*, as a dynamo or storage battery with a constant unvarying voltage, from which the current is conveyed to hospitals, physicians' offices or patient's houses.

2. A *current regulator*, rheostat or modifyer by which the strength of the current may be adapted to the required needs of individual cases, and this should be of convenient size, durable in construction and simple in arrangement. The simplicity, economy and efficiency of such appliance would insure its being readily adopted by the profession.

3. A *series of electrodes* which should be constructed according to a fixed standard as to size and shape, and they should be made of material which is cheap, readily conducting, and non-polarizable.

4. All should make use of a *standard milliampéremeter* for determining "strength of current" and a *colombmeter* for "quantity" or dose.

Complying with such conditions as these, progress in determining the limitations of electricity as a therapeutic agent would advance with rapid strides. The incandescent electric light plants with low electro-motive force are very common throughout the country. The physician and dentist finds the current extremely useful in his office. It illumines his apartments, it runs his motors, it lights the tiny lamps with which he explores the cavities of the body. It heats the water for his sprays and douches. It will not be long before it will be made use of by the many as it has been for some time by the few, for all the applications of electricity to the human body, since it does away with a host of difficulties that have up to the present been of such magnitude as to make electric applications either a burden or a farce.

RELATION OF GYNÆCOLOGY TO NEUROLOGY.

Read in the Section of Obstetrics and Diseases of Women, at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D. C., May, 1891.

BY WM. B. DEWEES, A.M., M.D.,
OF SALINA, KANSAS.

The intimate relationship which exists between diseases of the organs in the female pelvis and the general nervous system has not yet been elucidated. Though as early as 1858 Brown-Séquard pointed out the dependence of insanity, epilepsy, hysteria and other affections, upon organic disease of the female generative organs. Baker Brown put these teachings into practice and furnished clinical proofs of their correctness. He, however, fell a martyr to the professional bigotry which his doctrine excited. Now, however, that the rancor of that day has died away, his book on "The Curability of Certain Forms of Insanity, Epilepsy, Catalepsy and Hysteria in Women," can be read with interest and modified by the later revelations of science, his teachings can be followed with safety.[1]

[1] Dr. Charles A. L. Reed, of Cincinnati, O. Buffalo Medical and Surgical Journal, May, 1889.

The influence of the unhealthy uterus and ovaries upon the development of the diseases of other organs of the body by virtue of the neuric forces through the intermeddling of the complex generative nerve-circle, the sympathetic and the cerebro-spinal systems of nerves, is a matter which I am led to believe has received very little attention at the hands of practitioners as evidenced by the fact, that I mailed a carefully prepared letter of inquiry to two hundred and fifty of the most noted practitioners of the United States, and the only responses came from Dr. Charles A. L. Reed, of Cincinnati, O.; Drs. Barton Cook Hirst, William Goodell, James Tyson, John V. Shoemaker and R. J. Dunglison, of Philadelphia, Pa.; Drs. Trail Green and Charles McIntire, of Easton, Pa.; Dr. C. Henri Leonard, of Detroit, Mich.; Dr. Henry O. Marcy, of Boston, Mass.; Drs. J. Taber Johnson and Irving C. Rosse, of Washington, D. C.; Dr. R. H. Thomas, of Baltimore, Md.; Dr. P. Bryce, of Tuscaloosa, Ala., and Dr. Alexander B. Shaw, of St. Louis, Mo. I had hoped to be able to present valuable data from this source, but the responses being too limited in number, and too meagre with facts, to be of any material value to science, they have therefore been omitted.

For more than a decade my attention has been directed to the interdependence between diseases of the female pelvic organs and diseases of other organs. Which is the cause and which the result is not always possible to determine, the relationship evidently varying in different cases, for while in a large percentage of cases of neurosis in the female, interdependence of diseased conditions through the neuric forces could be distinctly traced and finally lucidly established, by the subsequent course and termination of those cases; in other cases the causes could be seen to depend upon a condition of malnutrition alone.

My authority for coming to this conclusion may fairly be questioned, and therefore I give my reasons. If I am consulted by a female patient complaining of voice, heart, brain or other troubles in whom examination reveals physical signs of diseases of these organs, and if in the same patient I find uterine or ovarian disease, and if the voice, lung, heart, brain and other troubles disappear upon treatment of the uterine or ovarian disease without any other medical aid, it is but logical to conclude that the voice, heart, brain and other troubles were dependent upon that of the womb or the ovaries. This being conclusively shown, by a few illustrated cases selected from a large number and appended hereto, it follows that what may have been a coincidence becomes a sequence, and the conclusion at which I have arrived is inevitable.

Far be it from me to assert from my experience that all cases of neurosis in the female are dependent upon lesions of the generative organs;

but that a very large percentage of the neurosis are dependent upon local disease within the pelvis is fully warranted. It is not my intention to theorize upon the subject, to waste useful time in useless conjectures, my object being only to draw attention to the clinical fact and to endeavor to awaken the medical mind to its great practical importance.

The hindrances to the true understanding of the relation of gynecology to neurology are to be found in :

1. The unsettled state of physiological and pathological history.

2. The absence of a thorough knowledge of the anatomy of the reproductive nervous system, and of data regarding the interdependence of menstruation and ovulation ; and last, but by no means least,

3. The existing ignorance concerning the neurosis attendant on lesions of the female reproductive organs.

Confronted with these facts we must acknowledge that we grope for the truth like the blind, and are at a loss for philosophical explanations based on historical and experimental research.

Every experienced gynecologist will upon reflection admit, that there is nothing very extraordinary in the implication of distant organs in connection with and dependent upon uteroovarian disease through the neuric forces, since in the practice of all such, instances are not wanting wherein tissue-changes have been produced at a distance by ovarian or uterine irritation. Medical literature is full of examples of acne rosacea, pharingitis, bronchitis, epilepsy, catalepsy, hysteria, melancholia, mania, insanity, diseases of the joints and muscles of the extremities, lesions of the heart and lungs, paralysis, etc., superinduced by reflex irritability of the intrapelvic organs. It is quite true that the number of women dying from these complications is now much less than formerly. We have learned better to differentiate and treat disease, but it is doubtful if we fully appreciate the consequental relations which one abnormal condition bears to another. While in many of these interdependent complications relative to the subject, the disease of distant organs bears to intrapelvic disorders the relation apparently of effect to cause, and you can observe for yourself the fact, yet the manner in which the changes are brought about may remain an open question.

Neurosis may be defined as an exaggerated intensity of both the sensor and the motor forces, and may be induced by *a*, developmental, or *c*, pathological causes.

a. Psychical causes may act by an impressionable idea allowed to become fixed by an active fancy or imagination, producing a diseased condition of the imagination, whencesoever, idealization produces neuric impulses.

b. Developmental causes may act by the rapid and swelling growth of the intra-pelvic organs as manifested by gestation.

c. Pathological causes may act by a fluxionary movement, as in suppressed menstruation ; the presence of the neoplasm of the ovaries or uterus; misplacement, engorgement or inflammation of these organs. Pathological causes may also be produced by muscular laxity of the abdominal or spinal muscles, or both—unquestionably the most prevalent forerunner of all causes—destroying the primitive erectness of the body, the normal obliquity of the pelvis, the maintained ascendent position of the internal viscera from below upwards, resulting in a mechanical displacement of the visceral status, involving a train of both physical and functional derangements of the pelvic organs.

All of these causes may and do pervert the normal forces of the complex generative nerve-circle to disturb the whole economy, and the persistence and permanence of these causes may develop the emotional influences to an uncontrollable degree, as evidenced by that undefinable neurasthenia known or designated by the term hysteria, or finally affect the mind and even destroy reason, as evidenced by melancholia or insanity. Thus we have the sympathetic neurosis, hysteria, as the enalogue of that cerebral neurosis, insanity, both alike dependent upon pelvic neurosis, which fact goes to prove conclusively the interdependence between gynecology and neurology.

In the language of Dr. Chas. A. L. Reed, of Cincinnati, O., who, speaking of the female organs of generation, says: "It may not be amiss, however, to recall the fact that all of these organs have intimate connection with both the sympathetic and spinal systems of nerves, and that these systems of nerves in turn communicate with each other. Through these instrumentalities morbific impulses originating in diseased conditions of the intra-pelvic viscera may be telegraphed, as it were, either to those centres which preside over nutrition, or to those other centres which govern sensibility and motion. In the former instance the sympathetic system furnishes the route of transit, and the result may be observed in the modified calibres of all the blood-vessels generally; in the perverted activity of all the glandular organs; in the unrhythmical movements of all the other viscera and gland ducts, or in a more or less marked interference with ultimate tissue nutrition. In the latter instance the cerebro-spinal system of nerves is involved and the results are pain, and may be motor disturbances of either the voluntary or reflex character. In either case the brain is reached, and we cannot fancy that impressionable organ receiving an impulse in excess of normal without sustaining more or less functional disturbance, whether that

disturbance be manifested by psychic phenomena or not. The perpetuation of this disturbing influence through the persistence of the initial lesion in the distant viscera is sufficient to convert the functional disturbance of the brain into an organic change involving that organ."

Here let me digress to state in this connection that I regard thought and all mental actions to be the results of brain elimination, and in proportion to the quantity and quality of the grey matter of the brain so will be the intellectual capacity of the individual. Any change, either progressive or retroprogressive, in the metamorphosis of the ultimate cell structure of the brain will upset that symmetrical balance of action known as sanity, and the various grades of insanity are developed. I regard the brain as the being—the man—all else, limbs and organs, as servants moved and compelled to do duty by the nervous mass. Oken asserts "that the animal is naught but nerve." Can you gainsay him? From the nervous mass called brain issue two, and only two forces, one sensor and the other motor.

Returning to our subject, it may again be argued that, first, there is a derangement and impairment of the complex nervous system and neuric function from the utero-ovarian or pelvic diseases, followed by impaired digestion and malassimilation as a natural consequence. Then comes the persistence of disordered hepatice, renal, etc., circulation, the invasion of the contents of the digestive tract by the *torula cerevisiæ* and consequent fermentation with its attendant disorders constantly increasing. Finally, by excessive irritability of the nerve-circle presiding over the orgasm function there is developed a morbid sympathetic or cerebro-spinal action, which may be curable because there is simply supernatural reflex irritability,—a sympathetic or cerebral neurosis dependent upon pelvic neurosis.

APENDIX OF CASES.

Case 1.—In May, 1886, I was summoned to take under care the wife of a farmer, aged 54 years, and the mother of two sons, who had passed the menopause at 46, and always enjoyed good health up to about eighteen months prior to my seeing her, when she received some injuries by being thrown from a carriage by a run-away horse. Her suffering then being confined chiefly to her knees, which had become very swollen and painful, presenting all the local characteristics of acute articular rheumatism, for which disease, (the husband stated), nine different physicians had treated her during the eighteen months of her confinement to bed, without any apparent benefit, aside from the temporary relief of pain. A timely and careful examination revealed a complete protrusion of the uterus, greatly congested, from the vulva of the patient, who was lying on her back, and both her knees swollen to three times their normal size, red in appearance and very painful to the touch, so much so that the bed clothes had to be supported above them. She was very much emaciated by her long suffering and confinement. I noted my diagnosis procidentia uteri, complicated with neuristhenic insufflation of both knees—hysterical knees. I reposited the uterus and retained it by proper mechanical support and painted the knee joints twice daily with oil of peppermint, which together with attention to diet and cleanliness constituted the entire treatment. The patient made a rapid recovery, she being able within six weeks to leave her bed and come to my office, a distance of ten miles, with all the appearances of being a well woman, and has remained well ever since.

Case 2.—Miss ——, aged 24 years, was supported into my office by her father and mother seven years ago. The history given to me of her case was that she had been then under treatment for consumption and hystero epilepsy for six years, without benefit, but on the contrary was gradually failing. The girl's appearance was pitiable in the extreme. Large drops of cold sweat bathing her emaciated face, which bore a livid hue, with an expression of extreme anxiety; her extremities swollen and abdomen distended until it had become painful. Of late she had been having frequent hæmoptyses with dyspnœa, painful chest and great increasing weakness. Back-ache, irregular and painful menstruation, leucorrhœa, anorexia, hacking cough, and a weak husky voice. She was now seized with a hystero-epileptic convulsion, and I had to defer further examination until it had passed off. After which auscultation revealed areas of dulness with sibilant ronchus over both lungs, prolonged expiration and feeble heart's action. Examination per vaginam showed a prolapsed retroflexed and inflamed uterus. The treatment consisted of special attention to the uteriue condition, rest in bed for several months, hot water douches and nourishing diet, with finally placing a Smith Hodge pessary, and her recovery was complete in six months, she being robust and well ever since.

RAPID DILATATION AND CURETTING.

Read in the Section of Obstetrics and Diseases of Women, at the Forty-second Annual Meeting of the American Medical Association, Washington, D. C., May 5, 1891.

BY J. G. CARPENTER, M.D.,
OF STANFORD, KY.

This is a subject of no little importance to the profession; the voice of warning has already been raised against, it might be said, the wholesale use of rapid dilatation and curetting or the practice of these in the hands of those who are unskilled in their use and are poor diagnosticians of intra pelvic disease; in fact, these therapeutic measures have been severely condemned by distinguished abdominal and pelvic surgeons, chief of whom is Dr. Joseph Price of Philadelphia. He has written a most excellent paper on "Certain Causes of Major Pelvic Troubles Traceable to Minor Gynæcology."

That they have been severely condemned is no argument against their proper use; that they have been shown to be instruments of great harm in the hands of the rotenist, the inexperienced, and unskilful, and met with so great censure by intra pelvic surgeons so eminent, will deter many from their former practice and save the female the ordeal of abdominal sections. It may be truthfully stated that when the intra pelvic affections threatening life, arising from rapid dilation and curetting, as well as the improper use of the uterine sound and trachelorrophy, and abuse of pes-

saries and caustic applications to the endometrium, are realized and appreciated by the so-called pseudo, or minor gynæcologist, there will be fewer cases with their ovaries and tubes in bottles, instead of their normal habitat.

Unfortunately for humanity so many graduates as well as senior members of the profession appropriate to themselves the stupendous title—Gynæcologist, and seem to think the essentials to a successful practice and fortune are the speculem, uterine sound, curette, Goodells' dilator and Thomas' retroversion pessary, and handle the sound with as much recklessness as the dude twirls his cane, and imagine all intra pelvic diseases are uterine. The ultima thule is fewer and better practitioners, less number of medical students, fewer and better medical schools with enlarged hospital experience, and instead of a three years course of study being compulsory, make it five years or longer, with abundant clinical or hospital experience and gynæcology learned and taught before it is put into practice. Unfortunately only the minority of every graduating class have had proper clinical experience to begin practice; the majority seldom reach mediocrity; they start wrong, go wrong and end wrong.

There are cases demanding rapid dilatation, or curetting, or both, and the physician would be direlect of duty not to use them. Their proper use must be commended, their improper use condemned.

The scriptural adage, "be temperate in all things" is highly appropriate and essential in the practice of minor gynæcology. That the minor gynæcologist too often ignores or is incompetent to detect existing intra-pelvic diseases when practicing these methods, or by the practice produces intra-pelvic diseases, there can be no doubt, just in the proportion to the abuse of rapid dilatation and curetting and other minor measures does he become a *feeder* to the *major gynæcologist* or *abdominal* and *pelvic surgeon*, only a few weeks observation and study and analysis of cases in the practice of men in the profession like Drs. Joseph and M. Price, Charles B. Penrose, E. E. Montgomery and Hoffman of Philadelphia, and T. A. Emmet of New York, would convince any "doubting Thomas" that "certain causes of major pelvic trouble are traceable to minor gynæcology."

One class of patients, you will find the major pelvic trouble traceable to a dirty confinement, or too early leaving bed and resuming household duties or hardships, or other minor imprudences; to imprudences during the menstrual molimen and frequent abortions; others have had a gonorrhœal infection or sexual abuse; others date origin of the ailment to an improperly adjusted pessary; to the use of the sound or probe; caustic application to the endometrium or os; others to operation on cervix (trachelorraphy); others to rapid dilatation or curetting, etcetera etcetera. That the general practitioner too often generalizes in place of specializing, and the specialist too often specializes instead of generalizing, there can be no doubt, and that the minor gynæcological tinker, tinkers with his patients until major pelvic troubles arise, and his patients pass into the hands of the pelvic surgeon *is self-evident*. These major pelvic troubles, many of them like Banquo's ghost, will continue to arise, because they will not down or cease until the pseudo-gynæcologist ceases to tinker. These questions may be asked, are forcible dilatation and curetting ever essential, and when are they indicated? Answer, yes. When and by whom should they be used? They should be used by the aseptic physician, or surgeon, who has been taught by practical experience and differentiation the pros and cons for these measures, and as a dernier resort; but some times, as in case of hæmorrhage and putrid placental infections, their use as means of primary remedial importance must be considered, in order to save life and prevent sepsis; and with the aseptic instruments, through the aseptic vagina, os, and cervix and when dilatation or curetting is accomplished, make the endometrium aseptic with hot water, the bichloride or boric solutions, iodine and glycerine mixture (Churchill's tincture of iodine,) the liquid vaseline and oil of eucalyptol, or menthol solutions, the insufflation of bismuth, subnitrate, or iodiform; the vagina kept aseptic "pro-re-nata" by hot antiseptic douches and aseptic vulva pad to prevent decomposition of the uterine discharge and infection. Before forcible dilatation or curetting are resorted to, the patient should be given a hot water and soap bath, purged freely, and placed in bed at rest twenty-four or forty-eight hours, and the vagina douched with hot antiseptics every six or twelve hours. After the operation, the patient should be kept in bed one or more days, until all manifestations of a local or constitutional reaction have subsided and the patient free from danger; the operation should never, never, be done in the private or consulting office, and never by the pseudo-gynæcologist; nor more than intervals of ten or fourteen days, at least five days before and after the menses, and when the disease is limited to the uterus *per se*, an entire absence of disease in the pelvis.

These measures, though sometimes indicated and though no intra pelvic inflammation is present, may and have done incalculable harm by setting up pelvic disease even though slight; no dilatation or curetting should be practiced unless absolutely required by patient to save life. Rapid dilatation or curetting are distinct traumatisms, and all the dangers incident to septic absorption may attend them that follow any other violent procedure. Dr. Joseph Price states in reference to rapid dilatation: "This conclusion reached

inferentially has been abundantly confirmed on the operating table by much of my later pelvic work. In a number of cases with a history of preceding dilatation, the after-operation has exhibited an inflammatory condition of affairs as complicated as any other; in my experience some of the dilatations were done with pre-existing disease which was made worse by the interference, while others were done simply to relieve the dysmenorrhœa and resulted in the establishment of a complicated surgical disease in which operation was necessary to save life. All in all, I believe that judged simply by its remoter effects, the operation of rapid dilatation is a dangerous one and results oftener in subsequent harm than in lasting good. The surgical injury to the cervix which it is the intention to remedy by Emmet's operation. Dilatation with curetting of the uterus have placed to their credit a long series of major operations."

The writer saw a unilateral laceration, left side, caused by the dilator, extend almost to the fornix vaginæ. No doubt many experienced physicians have dilated with good results, such men as Emmet, Gilwily, Goodell, DaCosta, Baldy, Ashton, Noble and others who are not so eminent, but they have been taught gynæcology in all its relations and know the when, and how and the pros and cons. Dr. DaCosta states: "Dr. Goodell has reported to this society (Philadelphia County Medical Association) many cases in which forcible dilatation has been used with grand results. I have used forcible dilatation in many cases and have never had bad results. The reason is that when I began the study of gynæcology I was taught how to use it properly, and not to use it in every case. The dilator is a surgical instrument and one which must be handled carefully. You must know how to do your work before you attempt to use it. My teaching is, when there is violent inflammation in the pelvis not to do any operation on the uterus and to hesitate to use the sound." (He should have said never use the sound.)

Dr. William E. Ashton: "The question of the use of the dilator depends on one or two facts. First, as to the condition of the uterine appendages and their surroundings; secondly, properly selected cases. I believe that when we have the pelvis perfectly free from local disease in cases where the uterus is strongly antiflexed and perfectly movable, and upon the introduction of the sound we find there is a point of intense pain at the internal os, we shall find in a certain proportion of cases that good results are obtained by the dilator. The vast majority of cases of dysmenorrhœa are cases which have a distinct tubal or ovarian origin. It would be absurd to rapidly dilate in such cases. It should go on record that the uterine sound should only be used by men who have a thorough knowledge of the pathology of the pelvis and who can appreciate the great danger incident to inflammatory troubles in the uterus."

Dr. C. P. Noble: "I should be loth to give it up (rapid dilatation); I have never seen any harm follow rapid dilatation in any case. This is due to the fact that dilatation has been used in cases in which the disease is limited to the uterus. I agree that it is useless and dangerous to dilate the uterus when tubal disease is present. In uterine disease it is capable of a great deal of good."

Dr. J. M. Baldy: "I should be loth to give up forcible dilatation in certain cases. It should not be done in every case of dysmenorrhœa, for the vast majority of cases are due to ovarian or tubal disease. I believe that in the vast majority of cases where trouble follows the use of the dilator, there has been pre-existing pelvic trouble. I do not think that a carefully done dilatation in a healthy pelvis will do harm. It is admitted that it will tear uterine tissue, but that this can cause trouble unless the wound becomes septic I am not prepared to admit."

Dr. Price, in refering to the article of a minor gynæcologist, states in debate (Philadelphia County Medical Society). "He is the great mischief doer. He tinkers, dilates, curettes, passes the sound and in four to six weeks he (Dr. Price) gets a telegram to come and open the abdomen to save the patient's life; that the woman is leaking; that she has a pulse of 130 to 140, with temperature of 104°. This recurs weekly; also stated of specimens in that jar removed during the month of August, 50 per cent. followed dilatation, closure of cervix, the use of the sound and currette. These specimens have come from four clinics in this city (Philadelphia), and from ten prominent gynæcologists. They all had sections to save life and all were greatly complicated operations. These fibroids in the jar had pus in them, the result of electricity. This private office work has a great deal to do with it. Many of these men are simply cervix feelers, and never find anything above it. There may be a mass larger than the uterus on one or both sides which they fail to find. They are not anxious to find them and would not be troubled by them or capable of dealing with them if they struck them accidentally."

This state of affairs, existing in Philadelphia. The havoc wrought by the minor gynæcologist could be doubtless estimated *ad infinitum* throughout the world. Dr. Joseph Hoffman states, I have put on record, in the Obstetric Society, a case where the uterus was perforated by the curette. I have seen to day two patients that have been treated by the curette and from whom I have removed the appendages. In one case that I know of the uterus was torn by the dilator, then a sponge tent was put in and allowed to remain, I

do not know how long. You know the rest. In the case in which the uterus was perforated by the curette, the operation was done by a gynæcologist of considerable experience. Nevertheless the uterus was ruptured and peritonitis was brought on and abdominal section was necessary to save life. I have to-day two other women who were treated by minor gynæcologists; they were both left miserable. In one the vagina is much contracted and the pelvic viscera are certainly affected. In one of these cases especially electricity was used ad nauseam. The history is this: first dilatation and scraping; then closure of the perineum; and then the opening of the abdomen. Dr. Emmet for a decade or more gave the danger signals of minor gynæcology ignorantly and thoughtlessly practiced, but the warning has only been heeded and appreciated by the few. The many, it might be truthfully said, have gone on their way in mid-night darkness causing pain and anguish, destroying important organs, wrecking constitutions and loss of life. Emmet's (Principles and Practice of Gynæcology) is replete with aphorisms of wisdom lightening the pathway all along the line and is to the major gynæcologist a "sermon on the mount," but the pseudo-gynæcologist or gynæcological tinkers are deaf and myopic and would not turn from the error of their way "though they had Moses and the Prophets to warn them." Dr. DaCosta states, "then in regard to the curette, these usually have a sharp cutting edge. Such an instrument is hardly safe for an able practitioner to use and is not safe in the hands of the unskilled person; when inflammation extends from the uterine cavity to the tubes, after the use of the curette, it is not so much the instrument as the man who uses it." Dr. Hoffman states "the curette in some cases is a necessary evil which we cannot do without. I have found it useful in getting rid of putrid debris from miscarrying uterus in the early weeks of pregnancy, when the use of the finger is thoroughly clumsy and painful; if not impossible, without previous dilatation with tent, I can say I have had no bad results I know of."

Dr. Baldy: "The curette, I think is a valuable instrument, but it is abused and used indiscriminately. After abortion I found it most valuable. In some cases of chronic endometrial disease it is valuable. I believe it will remove almost all necessity for intra uterine treatment. I think the dull curette useless. The only rational treatment is the sharp curette. The sharp curette can be used with as little danger as any other instrument if used properly in skilled hands."

Dr. C. P. Noble: "Why we should give up the curette, I cannot understand. There are many cases of hæmorrhage of the uterus due to uterine disease purely, where there is no ovarian or tubal disease. In such cases, the curette will permanently control the hæmorrhage." (The consensus of opinion is that the dilator and curette are valuable instruments in skilled hands, but otherwise are agents of potent evil.)

When so skilful and erudite a surgeon as Dr. Emmet has had harmful results follow forcible dilatation and curetting, with all his care, diagnostic and operative skill, painstaking and prophylactic measures, one is forced to believe inferentially that the gentlemen who have been quoted would certainly have had experience similar to Dr. Emmet had they followed up their cases, corralled them, and hunted for after-claps or evil results. I have found it often (uterine dilatation) very serviceable for arresting hæmorrhage, particularly when due to soft growths, or to some diseased condition of the canal. It is of great value after a tent has been used to increase the dilatation at the time of operating for the removal of an intra uterine tumor. Ten or fifteen minutes will often be sufficient to effect the necessary dilatation for purposes of exploration, but as already stated, rapid dilatation should never be employed unless the case is urgent. If pregnancy exists and a tent has already been used, or if there should be a hæmorrhage, there will be less risk from rapid dilatation. Under other circumstances, it is always attended with the danger of existing inflammation. (Page 36, Emmet's Principles and Practice of Gynæcology.) In hospital practice, when I could control the movements of the patients, I have long employed sponge-tents to bring about a reduction in the size of the often enlarged uterus. This mode of practice is original with me, and from long experience I have found it most satisfactory whenever employed under proper circumstances. The object is to bring about by pressure an alterative effect in the mucous membrane and indurated tissue, to excite contractions of the whole organ and to lessen the circulation in the uterus by means of the profuse watery discharge which is invoked. (Page 140.)

It is equally beneficial, if not more so, when there exists no enlargement, since the disease is then confined chiefly to the cervix, or at least to that portion of the canal below the internal os. By dilating the canal thoroughly in these cases, it will open at the rugæ, so that the agent applied can be brought directly in contact with the mouth of each crypt, and every portion of the mucous membrane. This cannot be done under ordinary circumstances, and the fact will explain the negative result, frequently observed after operations made through the undilated os, for the nearer the uterus approaches to a natural state the deeper will these folds be found. In reference to local treatment Emmet states: "We should always have the fear of cellulitis before us." No portion of the body has suffered more from the over-zealous interference of ignorant practitioners, and from the carelessness of those who,

though not ignorant, fail to make such a thorough investigation of their cases as is essential to their successful management. Under the guise of surgery, the uterus has been subjected to a degree of malpractice, which would not be tolerated in any other portion of the body. Its cavity has been and is to day made the receptacle for agents so destructive, that it seems difficult to understand how their evil effects have escaped observation. But I trust we have already passed the heroic age, and that in the treatment of uterine diseases, we may be hereafter governed by the same rational methods as should apply in every branch of medicine, that is, we may simply, as we term it in this country, exercise our "common sense."

Indications for Dilatation.—Dysmenorrhœa due to cervical stenosis; its cause should be reached by exclusion. There is sometimes primary stenosis; all conditions being normal; the mucous membrane engorged at the menstrual nisus, constricts the internal os, and when there is endocervical and corporeal mentritis in the virgin or nullipara, the stenosis may become much aggravated, especially if, in addition to the hypertrophy and hyperdistention of the mucous crypts with closed orifices and pent-up sections, there is hyperplasia of the submucous connective tissue. To one familiar with the occlusion of the nasal chambers from chronic and hypertrophic rhinitis, it can be readily perceived how a similar condition of the uterus mucous membrane could cause and aggravate a stenosis of the internal os, and how the displaced uterus in the form of visions, flexions and obliquities, single or combined, could cause stenosis like the flexed or dislocated portion of the nose, and be amenable to restitution and dilatation to straighten the uterine canal, making it more patulous; the pressure opening the orifices and emptying the distended crypts of their contents, allowing their walls to collapse, besides causing absorption of effused fluid and unorganized lymph, and by straightening the uterus, also straighten the arteries, allowing increased nutrition, by rectifying the veins, relieve passive congestion, removing the engorgement from the over-loaded capillaries, and in a similar way, increase the functions of the lymph channels. Not only may an enlarged uterus be benefited this way, but, also a uterus of natural size afflicted with endomitritis. When the walls of a displaced uterus are straightened by dilatation the womb is much more easily retained in normal position, gets more patulous at the internal os. Some operators would recommend dilatation for infantile uteri, to increase their nutrition and size, but we doubt the expediency and believe the weight of authority is against it, since their cause is tubal and ovarian. After dilatation the distended crypts can be scarified or punctured; dilatation is often essential for diagnosis of intra-uterine growths and foreign bodies, and as a preparatory step to curetting. Emmet speaks of the beneficial effects of forcible dilatation for malarial congestive hypertrophy of the uterus. We can also often obtain the pressure, size and positions of intra-uterine growths, and use it as a preparatory step to removal of these growths of foreign body. Dilatation is essential to wash out the uterus when there is sepsis, and in conjunction with curetting to remove septic material and arrest hæmorrhage. Rapid dilatation should never be used unless the cause or case demanding it is urgent. (Emmet states if pregnancy exists and a tent has been already used, or if there should be hæmorrhage, there will be less risk from rapid dilatation than under other circumstances; it is always attended with the danger of exciting inflammation.)

Indications for Curetting for Diagnosis.—Presence or absence of growths or tumors in uterus. Removal of portions from cavity of uterus for inspection. (Skene.) In chronic endometritis, the removal of granulation tissue, as in granular conjunctivitis to open the engorged and distended crypts, and relieve them of their contents and free the lumen of the canal; to remove fungous, or foreign growths, and retained septic placenta; to arrest hæmorrhage. In malignant disease of the cervix, after removing all growth possible with the scissors, the Sims curette, or Thomas' wire scraper, followed by the paquelin cautery, becomes a valuable adjunct. Emmet states Sim's scraper has too sharp a curetting edge and causes much bleeding. When the follicles of Naboth undergo cystic degeneration and are detected by their rough and uneven surface they should be ruptured with the curette, used with mild force over their surface. In diagnosis of intra uterine growths, the curette is claimed by some to be a valuable diagnostic measure, but it removes both diseased and normal tissue.

Thomas' wire curette, and Emmet's scissors-shaped or forcep curette are recommended as the safest and most expeditious. The dull or wire curette from the friction or pressure effect produced, may change the nutrition of the affected parts by retraction and absorption of exuberant tissue, and soft villous growths. In cases of old cervical lacerations with multiple papilloma and warty excrescences, the sharp curette has proven a most valuable instrument and did beautiful work by removing them; chronic anhydrous acid was then applied to the bases with the best results, and no evil sequelæ, in the uterus of a woman 70 years old, in the fall of 1889; patient is still living and enjoying good health. Emmet's forceps curette can only remove the growths projecting above the uterine surface; it crushes the diseased tissue in its immediate grasp; in addition, too, it can be used as a dilator when neces-

sary. Of the Recamier curette modified by Simpson, Simon and Simms, it is said that the ingenuity of man has never devised one capable of doing more injury. Women suffering with intrauterine growths are, as a rule, quite anæmic, and very susceptible to septicæmia and peritonitis after curetting. Emmet states that he has known peritonitis, cellulitis, and pelvic abscess and even death, to occur on removing these growths from the uterine canal with the curette, and in every instance the operator was dextrous in the use of the instrument. Inflammatory disease may extend by continuity of tissue into the tubes, ovaries and peritoneum from the uterus, producing peri- or parametritis, or these can be caused by intrauterine instrumentation, *per se*, or the dilator and curette. If uteri free from intrapelvic complications only were dilated and curetted under proper conditions, and were a thorough and intelligent, digital and conjoined manipulation made in every case, before instrumental treatment is used, pelvic disease would be detected, and non-interference would be the rule, and major pelvic operations to save life would be less frequent.

When the minor gynæcologist ceases to tinker there will be far more tubes and ovaries remain in the pelvis. It is to be hoped that the millennium is at hand, that the "goats will be separated from the sheep," and to the pseudo-gynæcologist the scriptural injunction proclaimed "depart ye cursed, I never knew you," will be fulfilled, and that woman will keep her normal uterine appendages instead of having them removed and placed in bottles. The more experienced and skilled the diagnostician is, the less often will he resort to dilatation and curetting; conversely, the more ignorant and unskilled the operator, the oftener will he use these measures and greater will be the disaster to health, happiness and life.

WHEN IS ANTISEPSIS A FAILURE?

Read by Title in the Section of Obstetrics and Diseases of Women, at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D. C., May 5-8, 1891.

BY JOHN ERETY SHOEMAKER, M.D.,
OF PHILADELPHIA.

Outside of a comparatively small circle of surgeons there are heard from time to time suggestions, which occasionally appear in print, that the system of "Listerism," so-called, is a failure. Strange as it may seem, it is not very uncommon to hear some one say that in a given case "every antiseptic precaution" was adopted, but the result was bad. The speaker would have you believe that he had done his part and that the system was at fault.

Now, it is worth while to consider briefly where the difficulty lies; and without entering the discussion of asepsis as opposed to antisepsis —absence of dirt versus sterilization of dirt,— without advocating special methods or dressings, attention may be drawn to practical difficulties which lead to misunderstanding.

A few men, like Mr. Tait, vigorously attack the theory of Listerism, while they themselves carry out the principles underlying its success. The reputation of Mr. Tait, however, rests upon his operative work, and not upon his opinions or his explanations. When in characteristic style he says (*British Medical Journal*, Sept. 27, 1890, p. 728): . . . . "The tone and attitude adopted by Sir Joseph Lister at Berlin clearly shows that the whole sad business is on its last legs," etc. Also (p. 729): "I venture to say that before the present generation has run out the word 'antiseptic' will be all that is left to represent this strange structure." The harm that he can do is not great among the men who are doing the best work in surgery, especially in general surgery. These cannot work for a day without discovering for themselves that their results are better or worse, according to their greater or less microscopic and chemical cleanliness in operating. Active surgeons do not care how Mr. Tait explains his good results. He might refuse to believe in the law of gravitation if he choose, but as long as he did not violate it, as long as he refrained from walking out of windows or off precipices, his opinion as to the law would make little difference. Most men care little that he denies the evil potency of germs and relies upon removing decomposable material from his wounds. They remember that he deals with a peculiar membrane and its neighborhood, that he is extremely clean in his work, and they will permit him to attack Sir Joseph Lister personally, and his impregnable principles, to his heart's content; principles of the widest practical application. The harm Mr. Tait can do is to unsettle the mind of the man who is beginning his work; and, worse than that, his writings tend to salve the conscience of those who have had no training in genuine aseptic methods, who fail consequently to fully carry them out, and who joyfully hail any champion who even seems to justify their indifference.

But even among the better trained class of men, does not one often see a lamentable failure to grasp the essential ideas of surgical cleanliness?

There are hundreds of men to-day who apparently persuade themselves that mopping a 1 to 20 carbolic acid, or 1 to 2,000 bichloride of mercury solution about a wound area constitutes using "every antiseptic precaution," as the phrase goes. There are also men who will use chemicals upon a septic patient but neglect to change infected bedding. There are men who will go to an operation with the points of their scissors, the locks and serrations of their hæm-

ostatic forceps, the eyes of their needles, choked with dried blood or worse material from the last operation. They never boil an instrument. Their conscience is satisfied with the carbolic acid in the instrument pan. Some men wash their hands before an operation no better than before dinner. When an instrument or a sponge drops to the floor they may rapidly rinse it in the pan and use it at once. There are other men who have trained nurses, sterilized dressings and boiled instruments, but who, after they have washed for the operation, shake hands with a spectator, put a hand in a pocket, remove instruments from an old blood stained case, help carry a table, handle dusty bottles, or use a handkerchief, and yet *say* they use every antiseptic precaution. Many men know better. What is lacking is careful self-training and what may be called an aseptic conscience. What is wanted is a realizing sense of the real difficulty in getting things clean and then keeping them so. Carbolic acid solutions as practical sterilizers are a delusion and a snare. They work slowly at best. Unless too strong for comfortable or safe handling they do little good, and they do enormous harm by quieting the conscience of the man who ought to spend more time cleaning his hands; yet how many times do we see them relied upon when they only cover dirt. Many text-books, even the revised editions of standard works, are written from a carbolic acid standpoint. Antisepsis is a failure when it is superficial.

In a recent case of laparotomy, referred to by the author's permission, the stitches cut out and the wound opened, though it afterwards united by granulation. The operator had been cleanly, but the fault was traced to a nurse who had handled the previously sterilized silk with infected fingers. In another case the paraphernalia were elaborate and the preparations minute. The chief assistant seized a falling ether bottle, old and very dirty, and without the slightest effort to cleanse the hand again it was soon in the abdominal cavity. The patient died two days later of peritonitis, which may have been a coincidence.

A few days ago, in a hospital, a major operation was in progress. The lecturer had dilated upon the beauties of the antiseptic methods. The catgut ligatures proving defective, he called for silk. There was a scurrying of nurses and an ancient open box of silk was brought. A spectator with unwashed hands threw a card of silk into the instrument pan, from which a piece was taken when scarcely wet and placed on one of the largest arteries in the body. A weak link breaks a strong chain.

In a hospital case of my own, requiring careful dissection about the face, an assistant, unknown to the operator, obtained an instrument which had been used a few minutes before in opening a suppurating bubo. In two days the wound area was an abscess under sterilized dressings.

Not long ago, in an emergency, the writer asked at a drug store for antiseptic gauze. The druggist instantly opened a beautifully decorated and labeled tin box, unrolled quite a quantity of gauze and offered it for inspection. He was, of course, told that while that might have been antiseptic gauze once it was ruined as such by his handling; a proposition which failed completely to enter his mind. No doubt that identical roll of gauze will be retailed, and the writer fears that there are physicians who would buy it measured by the yard on his counter, and yet hardly realize that it was worthless as a clean dressing or packing. But why multiply instances to show that "antisepsis," when neutralized by some single mistake, is a failure. Which should be blamed, the system or the application?

The war about asepsis as opposed to antisepsis is a minor issue. The great fact remains that the principles of cleanliness, though adopted theoretically throughout the world, are really carried out very imperfectly by most nurses, most hospital internes, some general practitioners in town and country, even (must it be said?)by many otherwise most excellent and estimable surgeons.

This is not the place to bring forward the overwhelming evidence in favor of surgical cleanliness in saving life and promoting swift recovery from operations impossible without it. This work has been done again and again. Many of us see it daily.

Let no one be misled by the war of methods into suspecting the truth of principles. Let each of us train himself constantly to make his work clean. Only by long practice can this be well done.

In regard to the use of chemicals, it is known that in abdominal operations they are not necessary. In general surgery, including railroad and machinery accidents, better results can be obtained by the use of sublimate. Perfect asepsis, though it should be aimed at, is almost impossible as a practical measure. The assistant of the moment is often untrained, and can neither be relied upon nor narrowly watched; nurses may be new to the work, derelict or incompetent; the wound is frequently infected before it is seen. The best results in general surgery are obtained with least trouble by combining the aseptic with chemical methods.

Further, let no man venture to criticize methods which he has never fairly tried; let him also bear in mind that his trial, though honest, may be superficial, and therefore faulty through his lack of patient personal training.

In conclusion then, in answer to the question: When is antisepsis or asepsis a failure? one may say, never if real, always if imperfect.

There is no doubt that the great principle of

cleanliness in surgery, whether obtained by soap, hot water, dry heat or chemicals, has come to stay, and the sooner all of us act thoroughly upon that principle, ignoring personal discussion, the better.

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PATHOLOGICAL ANTEFLEXION OF THE UTERUS.

Read in the Section of Obstetrics and Diseases of Women, at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D. C., May, 1891.

BY ELIZA C. MINARD, M.D.,
OF BROOKLYN, N. Y.

"Plastic operation to straighten the anteflexed uterus."

Accepting the opinion held by the most advanced gynecologists, that anteflexion is a deformity, and belongs to the group of pathological conditions which govern that order of lesion, there will be no place to discuss the old terms and divisions, which have controlled the discussions and treatment of anteflexions for a term of years past.

That this lesion is amenable to the laws and rules which govern the treatment of deformities, is self evident.

Any malformation is not usually meddled with unless it disturbs the mental or physical balance of the wearer. It must be malignant, painful, unsightly, or interfere with personal comfort or happiness.

The discovery of this deformity comes after puberty; and whether the cause is congenital non-development from malnutrition, or acquired, through inflammation, or from the result of other affections, such as adhesions, pressure or tumors, the lesion is a fact, the symptoms and suffering a certainty. The mischief is done before the case reaches the surgeon.

The points to be considered when such a case presents itself are: the amount of deformity, the complications, and the direct or indirect effect which it has or will have upon the health, circumstances and happiness of the individual.

The family physician has exhausted the mechanical modes of treatment taught by his college professor, for dysmenorrhœa, endometritis, pelvic peritonitis, diseases of the tubes and ovaries, partial stenosis and, if married, sterility. He has witnessed the failure of the drugs to relieve which are extolled so highly and which bear the trademark of our own and foreign chemists; for professors still teach that flexions are normal conditions, and are not to be considered of much account in pelvic lesions.

The gynecologist begins at the beginning, and pursues the beaten course of treatment; dilatation—rapid or slow—curetting, drainage, posterior section, with glass, rubber, or silver wire stem pessary, intra- or extra-uterine support, and operation for imperfect invagination, without success. Months and years pass, surgeons come and go, till at last abdominal sections for diseased tubes give the science-tossed sufferer rest, if not here, in the grave.

Accepting the three forms of flexion: that of the body, the neck, and that of the body and neck, I have selected the third form to illustrate the plastic operation which I believe will work much good to suffering womanhood, and if done in time, will prevent the necessity of abdominal section in some cases. I take this because it came to me unsought, and the greater will include the lesser in most treatments.

The operation known as "a plastic operation to strengthen the uterus," by E. C. Dudley, of Chicago, which has been so clearly and ably described in the February number of the *American Journal of Obstetrics* of 1891. This case, which has proved so successful in my hands—and my first one at that—is a typical one of a severe type, taken after others had failed and hospitals refused admittance.

Mrs. M. C., aged 25 years, has been married six years without having children. She had been a stout, rosy girl before marriage, and had menstruated without pain or discomfort for three years before her marriage. Three months after marriage she began to have pains, and the flow became scanty. She was compelled to go to bed every month. Gradually she grew worse and lost flesh. She took on an anxious, scared look, though of a happy, cheerful temperament when she aroused herself. Her home surroundings were easy, and her husband kind and anxious. The desire for children and the great suffering induced her to seek treatment after six years of marital misery. She had followed treatment faithfully for six months without improvement save in general health. When she had spent her little savings, she sought treatment at my free clinic. The dysmenorrhœa had become well-nigh maddening, and life had become a burden. She came to the clinic at the summer vacation, and my substitute, Dr. Mary W. Faunce, treated her for a severe pelvic peritonitis. On my return I found her quite well of the inflammation, and as I had had reasonably good success in this line of uterine lesions, I prepared to dilate. Upon careful examination I found anteflexion of the third form—flexion of body and cervix—of an acute angle, and fundus resting upon the bladder. Any attempt to pass the sound caused great pain, as the point of the sound impinged against the posterior wall of the canal. When the cervix was well pulled back only a small sized sound would pass. As it was near her monthly period, I resolved to dilate and drain. As she lived out of the city, she made arrangements to stay in town, and with the usual aseptic precautions, and under cocaine, I did rapid dila-

tation. It was tedious work to introduce the first instrument. It was done thoroughly, and with ⅓ of a grain of morphia in form of suppository; she left the office to remain in bed at a friend's home. She returned the next day looking pale and nervous. I introduced another suppository, gave her a tonic, and she left for her home. She wrote me a happy letter, saying she had passed the monthly period without pain for the first time since the third month after her marriage.

After the next period she came to my clinic, saying she had suffered more severely than before the dilatation, and she seemed so broken up that I sent her to my office from sheer pity. Before the next period I had her come into town and remain for a week. I dilated the second time, and put in an Onterbridge's silver wire stem pessary, under the same aseptic conditions and with cocaine. The stem remained three days, when uterine contractions forced it out. She remained in bed, and suffered from the stem so much as to require hypnotics and narcotics. She passed the period in bed, without pain, and a full flow, lasting three days. There was some pelvic tenderness, which passed away under treatment. She returned home, and gained in flesh and spirits. But her experience had been so unhappy that she sought hospital treatment, and wrote me to meet her at a New York hospital of note.

The physician in charge met us kindly and appointed a time for admittance. It was here, while discussing the case, that the assistant physician mentioned this plastic operation of Dr. Dudley's, and agreed to do it. When the time came for entering, an examination was made by the visiting physician, who decided that the case would be too tedious, as there was some pelvic peritonitis, and there were more urgent cases needing the bed, and she was sent home. I was thoroughly disgusted, and hoped I had been relieved of further anxiety. But she returned to me, stating that her husband had decided with her, that as I had given her the most relief, they wished I would do the operation discussed at the hospital, or any other operation I deemed proper, for her relief, as she feared she would be driven to suicide if it went on longer.

I called in consultation Dr. Faunce, who had seen her first, and described the operation I wished to do and sought her help. She entered into my views with enthusiasm, and we made ready to operate. Upon a thorough physical examination we found the left ovary and ligament quite tender, and an abdominal aneurism as complications. The heart at times was heavily overworked, but no organic lesion.

The uterus was of normal size, high up, lying over upon the bladder, and the cervix bent forward, making an acute angle. The neck measured 1¼ inch in length to the bend, which was at the internal os. There was a profuse discharge and a severe endometritis. The sound would not pass. The anterior lip was long, the angles of flexion were immovable, and the cervix could not be straightened out. The anterior wall was thinned, and there was evidently obstructed circulation. The overburdened heart was acting in sympathy with the disturbed nervous action. The case was not an encouraging one. The *Obstetrical Journal* arrived at this junction, and Dr. Dudley's article, so ably written, was scanned with intense interest. The more we discussed and read the more we decided that this was the operation, and our enthusiasm increased. The patient aided us by her courage, and urged us not to wait over another period. The complication of the aneurism damped our ardor somewhat, but after a week's rest and treatment, we put her on the operating table, and with the usual aseptic care, etherized, dilated, curetted, washed out the cavity with solution of carbolic acid; then divided the posterior lip with scissors to the junction of the wall of the vagina with cervix, and carried the incision up to the interior os with the bistoury, then bent the cut surface upon itself and stitched the external os to the point of the cut angle near the internal os, with silkworm fibre.

We cut out a portion of the anterior lip from the external os to the vaginal wall, and enclosed the wound. The operation resembled a fine lacerated cervix which had been done in an aseptic manner. A Peasley sound entered readily, and a new canal, pointing backward, formed an entrance into the uterus at a slight curve. The uterine discharge ceased from that day.

She rallied from the ether and operation well. She did not vomit, nor was much sick. The usual toilet and diet was given. The aneurism gave some trouble and much anxiety, and was controlled by opiates. The operation gave no trouble—the stitches were removed the eighth day. The healing was good; the Peasley sound passed easily without much discomfort. She was kept in bed till after her monthly period. She was kept under observation till the second period, when she was allowed to return to her home, and a letter came before I left, stating that she was living a new life, and had had but one heart attack.

The limited time will not allow me to discuss causes and reason for operation. The author of the operation has so candidly and conservatively done this in his paper that there is no need to do so. All will receive gladly any added treatment which will lessen the suffering of the dreaded dysmenorrhœa and that of sterility. I would not have the old treatment ignored. All means should be used to cure. It is as easy to do as a posterior section, adding a few minutes for stitching; and more safe with its aseptic wound than rapid dilatation. In well chosen cases it must succeed more or less, whether the indications are wholly or in part mechanical.

Whatever may be the complications, there can be no harm in making a clean, open canal for the easy flow of the menstrual fluid; to place the uterus upon a higher plane, and relieve the pressure of the rectum and the irritability of the bladder.

I will be pardoned if I am seemingly a little over-enthusiastic for such relief to my suffering sex, and if I place the author of this operation—though wholly unknown to me—among the other benefactors of my sex, Sims, Emmet and Sir Spencer Wells, it is because as a physician I appreciate results.

THE RELATION OF ATMOSPHERIC CONDITIONS TO INTERMITTENT FEVER.

Read by Title, in the Section of Practice of Medicine and Physiology, at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D. C., May, 1891.

BY HENRY BIXBY HEMENWAY, A.M., M.D.,
OF EVANSTON, ILL.

Among the various State Boards of Health, perhaps none have done more to raise the standard of the medical profession, and to defend the public against impostors, than that of Illinois. None has probably done more for the science of medicine than that of Michigan. What is said of the Boards, applies especially to their executive officers, who have given the distinctive character to the bodies of which they are members. Dr. Henry B. Baker's laborious care in collecting statistics, and zeal in spreading information are well known, and his work is highly valued. Like other mortals, he is liable to errors of judgment, and we fear that in such an error originated his articles on "Malaria and the Causation of Intermittent Fever," published in THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION (Vol. xi, p. 651, etc., and Vol. xv, p. 561, etc.), having been read before the appropriate Sections in 1888 and 1890.

The first article, coming from so high a source, commanded attention; its repetition demands the most careful investigation. If the conclusions are fallacious, the fallacies should be clearly exposed. If his reasoning is correct, the discovery is important, for it shows that the individual can easily protect himself from the disease by wearing suitable clothing, and keeping his surroundings as even as possible—keeping the house warmed by fires at night, for example. On the other hand, coming from so high an authority, by many the ideas will be regarded as facts until proved otherwise.

The articles in question do not show the judicial spirit of impartial investigation usually found in Dr. Baker's studies, but rather show a labored, and, as Dr. Lee characterized it when the article was first read (THE JOURNAL, Vol. xi, p. 653), "ingenious" argument to demonstrate a pet theory. They show how unconsciously a careful student may sometimes be misled and blinded by impressions. That the results reached are consistent, neither with each other, nor with facts, we shall try to point out.

His first conclusion is that "intermittent fever is proportional, either directly or indirectly, to the average daily range of atmospheric temperature." This sounds well, but reminds one of a test for flour once given by a good lady: "Take a pinch of the flour and throw it against the wall. If it is good it will stick—or else it won't." If the proportion is sometimes direct and sometimes inverse, does it not seem that any argument based upon it lacks stability? If we grant that this double relationship is a possible cause of the fever, has the author presented sufficient proof of the existence of such a fact? The proof is in the form of tables and diagrams, which we will examine.

Figure 1 of Dr. Baker's series shows that during the late war, the number of cases of intermittent fever in the United States Army, and the average daily range of temperature, both increased up to May. In June we find a much greater *rise* in the curve of cases, but a fall in the daily range of temperature. In July there is a steep decline in the curve of cases, but a rise in the other line. The daily range of temperature continues to rise until, in August, it reaches its highest mark. From July the number of cases increases until September. From their highest points, both lines decline until, in December, they touch bottom. It will be seen that part of the time the lines go in the same direction, but that during much of the time the case line variations are a month behind the other. This Dr. Baker explains by saying that it is due to the accumulation of old cases.

There are two elements of fallacy in this table. Reference to figure 2 shows that the line representing the daily range of temperature in Lansing is very different from that for the average of the State of Michigan. The case line in Fig. 1 represents a wide range of country—wherever the National troops were stationed. Some were in Texas, some suffered in Florida. Many shook at Charleston, and some were taken sick in the Northern cities. If Lansing atmosphere, in the interior of the State of Michigan, is not to be taken as a basis of comparison for the State of Michigan, how much less should the sickness in the whole army be compared with the atmospheric conditions around a single building, in one corner of the battlefield.

Secondly. Fig. 1 is a partisan table. Such a study as we are now engaged in is not a control

NOTE.—For tables and diagrams 1 to 12, inclusive, see Dr. Henry B. Baker's paper on "Malaria and the Causation of Intermittent Fever," published in this journal, Vol. xi, p. 651.

experiment. If it could be shown that intermittent fever increased and diminished, either exactly with or inversely with the daily range of temperature, keeping other conditions stable, one might perhaps be justified in making deductions therefrom. Since other conditions cannot be kept stable, they must be carefully examined, and eliminated as possible elements before drawing conclusions, especially if the variation in case and daily range of temperature lines do not bear a constantly definite relationship to each other. It must be remembered that *post hoc* is by no means always *propter hoc*. We would therefore have expected at least a report on the mean monthly temperature, and should have liked to have seen some reference perhaps to the mean monthly maximum and minimum temperature, and absolute and relative humidity of the atmosphere.

From the "Tables of Differences of Mean Temperature," published in "Smithsonian Contributions to Knowledge," I obtained the average monthly temperature of the following fourteen points, intended to fairly cover the field occupied by our army: New York City, Baltimore, Washington, Charleston, Cincinnati, St. Louis, Chicago, New Orleans, Knoxville, Atlanta, Louisville, Fortress Monroe, Ft. Morgan (Mobile), and Galveston. From the same source I obtained the mean daily range of temperature at Ft. Morgan (Mobile Point, Ala.), Washington and Philadelphia, and from Dr. Baker's paper I took the daily range of temperature for Memphis. These reports each represent a series of years and are substantially correct. I could wish that, as for daily range of temperature, I might have found more data available. From the means above mentioned I obtained mean daily range of temperature and mean temperature by months, for comparison with the number of cases of intermittent fever in the army, as shown in table and diagram 13. It will here be noticed that the daily range line rises from the low point in January to the highest point in May. From May it slowly declines about a degree to July, and then rises less than half a degree to September, from which latter point it declines to January. The mean temperature curve rises to its highest point in July, remaining nearly stationary until August, and then declining to January. The case line starts to rise from December, and reaches its first crisis in June, from which it falls to July, and then reaches its highest point in September. Its low point is before the low point of both the mean temperature and daily range. Its high point is four months behind the daily range crisis, and two months after the highest mean temperature. If the disease is the *direct* result of atmospheric conditions of temperature, we should expect its crisis to correspond with the crisis of one of the other lines. If the fever is an *indirect* result of the temperature conditions, we should expect its crisis to follow the crisis of the temperature, which it does in this case. The fall in number of cases from June to July, without corresponding changes in either of the other lines, clearly intimates to the student that there is some other element which must be considered. This I think is humidity, and perhaps ozonic changes also have an influence.

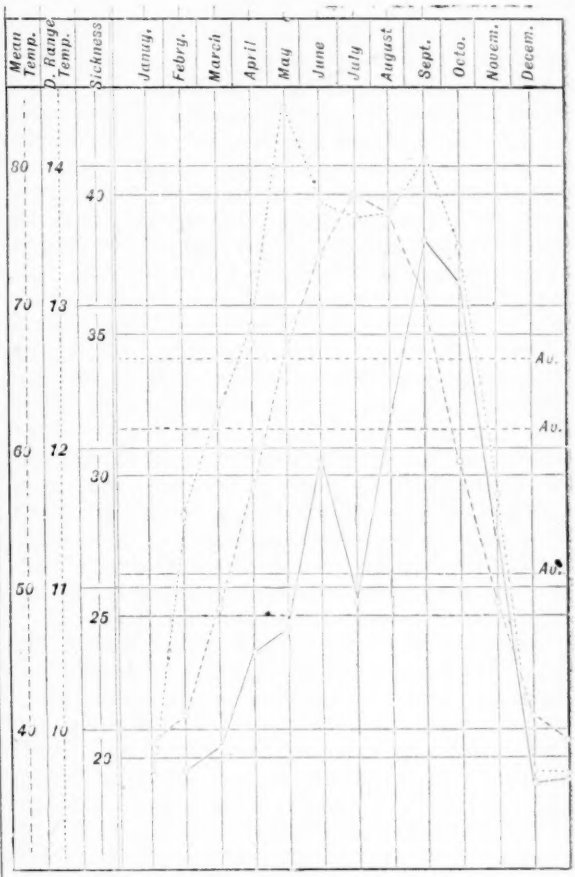

Diagram No. 13, showing average temperature (14 stations); average daily range temperature (4 stations). Cases per 1,000 intermittent fever in U. S. Army. (Yearly means horizontal lines.)

That the fall in the daily range of temperature from May until June is not responsible for the fall in number of cases, is shown by the fact that even in July (giving the advantage of two months fall) the daily range of temperature is *far above* the average for the year, but in July the number of cases of fever is *below* the yearly average. Further: although the case line from June to July is about five-sixths of case line from May to June, the daily range of temperature line from May to July is only about three-sixths of the daily range line from April to May.

A comparison of figures 2 and 3, clearly intimates that the disease in question is much more closely related to the absolute temperature than to its daily range, for the per cent. of reports of intermittent fever in Michigan has its highest

and lowest points exactly corresponding with the maximum temperature, and throughout the lines do not antagonize. On the other hand, from February to March the average daily range of temperature decreases without a like change in the case line; and the daily range line reaches its climax two months earlier than the case line, and the lowest point for daily range is a month before the lowest number of cases.

In studying Michigan tables, it must be remembered that the per cent. of reports does not necessarily represent the per cent. of cases. Blanks are sent to correspondents of the State Board of Health, asking them to report simply the presence or absence of the diseases mentioned. If ninety per cent. of these correspondents have only one case each the report would be greater than if seventy of every hundred averaged five cases each and the other thirty per cent. had none. The importance of this word of caution may be seen from a study of the annual reports of the State Board of Health. They divide the State into eleven divisions, for the ten years from 1877 to 1836 the reports of intermittent fever from the upper Peninsula were only 9 per cent., while from the Northern Division of the Southern Peninsula they were 27 per cent., and from the Southwestern Division 81 per cent., and from Southeastern Division 71 per cent. In the Southeastern Division in a single month there are probably more cases of intermittent fever than during an entire year in the upper Peninsula.

From the Michigan State Board of Health Report for 1888, I compiled table and diagram 14, showing the mean monthly temperature, absolute and relative humidity, day and night ozone for a series of years, compared with the per cent. of reports of intermittent fever as given in Dr. Baker's article. It will here be noticed that the curves representing the mean temperature and absolute humidity correspond closely with the per cent. of cases. Secondly: That in the main as the per cent. of night azone increases the per cent. of cases decreases and *vice versa*. Thirdly: The same is true of day ozone, though not so exactly. Lastly: The relative humidity does not seem to have any relationship with the per cent. of cases.

The reports for Memphis, Tenn., (table 4) strengthen the idea that the absolute temperature and not the daily range of temperature is instrumental in the production of the fever. The mean temperature and number of cases both reach their climax in July, two months after the greatest daily range of temperature. There are, moreover, decided changes in the daily range line without any appreciable effect upon the number of cases.

In Cincinnati (table 5) the climax in the daily range line comes in May. That of temperature in July, and that of cases in August. We find a great increase in the number of cases following the spring floods. There are very great variations in the daily range line without corresponding variations in the number of cases.

The San Francisco diagram (No. 6) shows all lines reaching their highest point in September, but here again the case line follows the mean temperature more closely than the daily range. The relationship of the lines, especially from May until August, clearly shows that there is some other factor besides temperature which produces the disease.

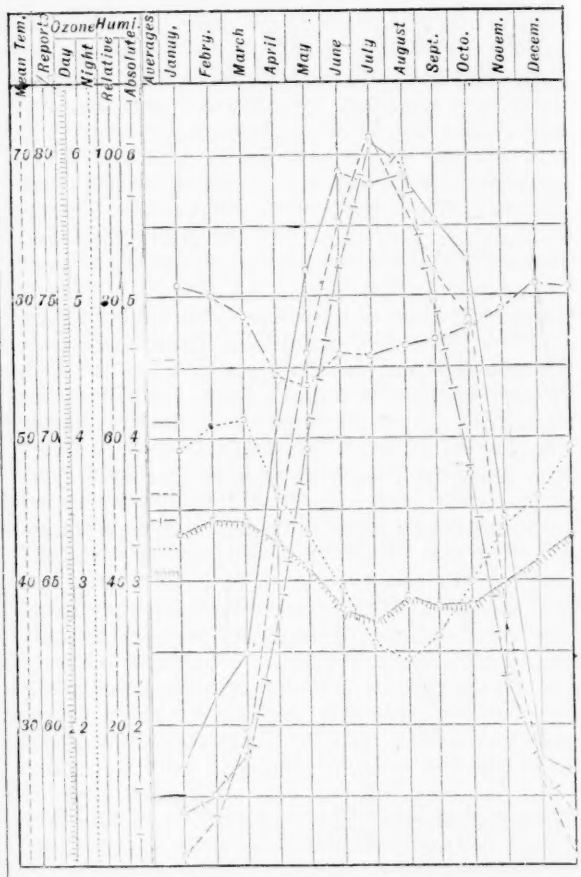

Diagram 14. Michigan. Showing average, 1, temperature (10 years); 2, ozone. day and night (10 years); 3, relative humidity (9 years); 4, grains of vapor per cubic foot of air (10 years); and 5, per cent. reports intermittent fever.

Diagrams 7 and 8 representing New York City and State, each show daily range line highest in June. Mean temperature highest in July, but the highest number of deaths from intermittent fever in September. It must be noticed that while other tables all refer to *sickness*, the data here refer to *deaths*. Since sickness must precede death we should expect the greatest number of deaths at least one month after the highest temperature, if high temperature is the cause of the disease.

Evidently the meteorological data in the State diagram are those of New York City. The daily

range is exactly the same as in the table 7, and the variation (less than half a degree) in the mean monthly temperature arises from the fact that for one table (No. 7) the figures are obtained by taking the reports from 1870–83, and in the other (No. 8) the reports used are from 1871–85. Believing that this did not represent the facts, New York City being about 150 miles south of the centre of the State, I prepared table and diagram No. 15, using for comparison reports for a series of years at Mohawk,[1] which is almost exactly in the centre of the state.

In table 9 we again find the greatest daily range of temperature in May, four months before the greatest sickness, and the highest temperature in July. This table is probably the most nearly correct of any in the series which cover more than a limited area. It illustrates, however, how difficult is such a table to prepare. If we imagine ten posts placed upon the same meridian, and each one of the nine southern posts having 1000 men, while the one northern post has 9000, the mean temperature of the ten posts would not give the average temperature surrounding the men. To get the correct figures the mean of the nine southern posts must be averaged with the one northern post. Here, as in the New York tables, it is to be regretted that the Doctor did not state his authority, giving the names of the stations used.

Tables 10, 11, and 12 quite contradict Dr. Baker's previous argument, and tables 10 and 11 representing India, also show that high temperature does not produce the disease. Here we find the highest temperature in May and June, the greatest daily range of temperature in February, and the least in August, but the most sickness in November.

If we suppose the daily range of temperature is equally above and below the mean temperature, we may get approximately the mean maximum and minimum temperature. This may be seen in table and diagram No. 16.

It will be noticed that in most places heretofore studied in this article malarial fever is most common at a temperature between 60° and 85° F., especially at about 70°. Neither extreme heat nor extreme cold are favorable for the disease. A glance at diagram 16 shows that until April the mercury every day went below 60°. From April to June each day it went above 95°. As soon as the minimum went above 60° the cases of sickness increased, and when the maximum came down to 87° there was a much greater increase. Every gardener knows that the young plant requires much more favorable surroundings than it does at a later period. An occasional high or low temperature would kill the young plant while the old one might thrive. So while a temperature of 56° does not favor the development of the fever, neither does it stamp out the developed disease.

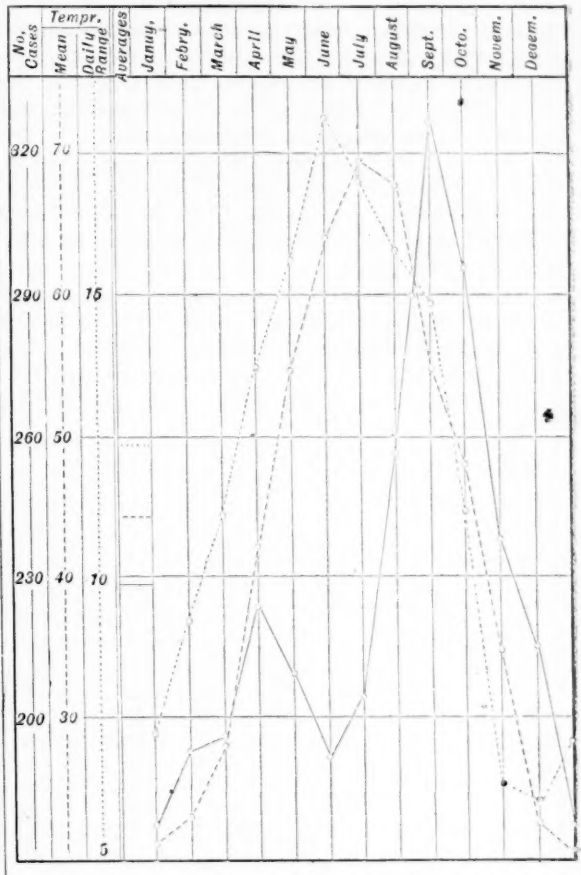

Diagram No. 15, showing number of deaths from intermittent fever in New York State, and mean and daily range of temperature at Mohawk.

There is another potent factor not shown in these tables. In "Hirsh's Handbook of Geographical and Historical Pathology," Vol. 1, p. 257, we read: "In the malarious regions of the tropics the fevers appear as a rule at the beginning of the rainy season. They increase in extent and severity with the increasing rainfall, remitting usually at the height of the rains, especially if they be very heavy, and reappear towards their cessation or directly after the rainy season, which is, as a whole, the season when the conditions are most unfavorable to health." Again, on page 269: "The production of malaria takes place on the largest scale where the soil is constantly saturated or periodically inundated." A friend residing in Lucknow informs me that from about June 15 until September 20, it rains almost daily, but from September until June all the rain put together would not make a good Michigan shower.

[1] "Tables of Differences of Mean Temperature" in Smithsonian Contributions to Knowledge.

NOTE.—I have searched for meteorological data for India, but did not find more than generalizations. I have therefore been obliged to use the tables of Dr. Baker—his authority not stated.

Table 12, to my stupid mind, shows nothing, unless it be that the daily range of temperature has *nothing* to do with the disease. As drawn, the lines coincide in but one place, August. There are most marked changes in the daily range, without a corresponding variation in the number of cases. Not being able to obtain the mean temperature of Ft. Apache, I took that for Camp McDowell,[2] shown in table and diagram 17.[3] Camp McDowell is near Ft. Apache, and practically on the same isothermal line.

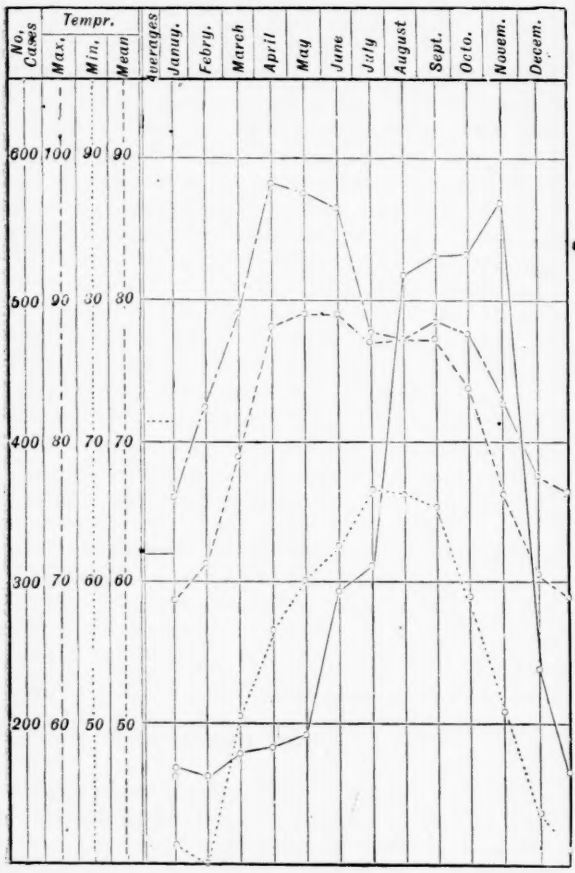

Diagram No. 16. India. Showing average maximum, minimum and mean temperature, and cases of intermittent fever among native troops.

Dr. Baker's second conclusion is that "the controlling cause of intermittent fever is exposure to insidious changes, or changes to which one is unaccustomed, in the atmospheric temperature." If this is true, why is the disease so much more prevalent in hot countries than in those that are cooler? If this is true, why does not the sickness follow closely the daily range? This many of the diagrams studied show that it does not. A patient under the care of the writer since this article began, was exposed to decided variations of atmospheric temperature. For two days she was obliged to be where the temperature was quite uniform through the twenty-four hours. She had no chill until after these two days of unusually even surroundings. This case is illustrative of many.

"The daily range (of temperature) diminishes from about lat. 40° in either direction, north or south. The precise latitude of maximum range cannot yet be given."[4] If conclusion two is true, there would be more malaria around Philadelphia

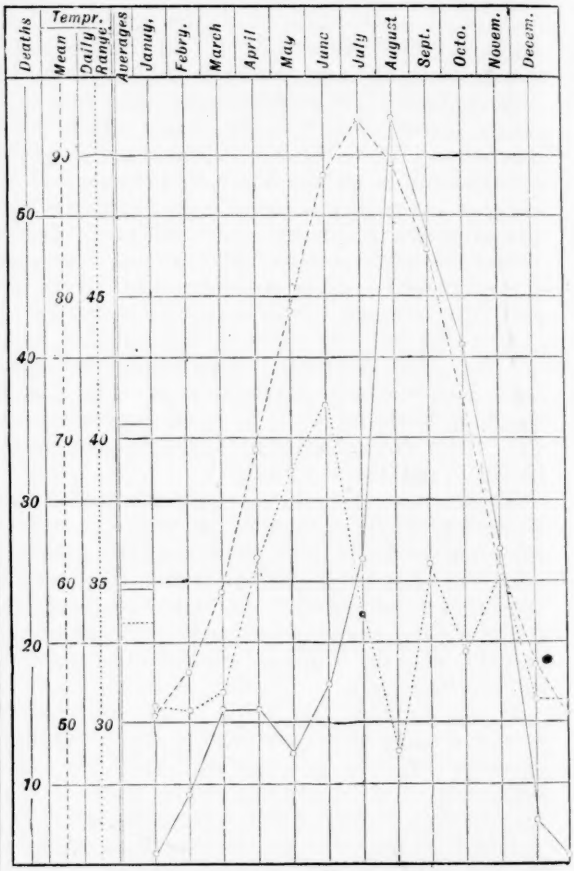

Diagram No. 17, showing average temperature at Camp McDowell, Ariz., and daily range temperature, and cases of intermittent fever at Ft. Apache, Ariz.

than around Charleston—more in Connecticut than in Florida. Is that the fact? If so, I for one am greatly mistaken. In Michigan,[5] the average daily range of temperature at Detroit is 15.79°; at Kalamazoo 16.93°; at Marquette 17.20°; Traverse City 19.13°. If the disease is due to daily range of temperature, we should find more of the fever around Marquette than near Kalamazoo, and more near Traverse City than in Detroit. I do not understand that to be the fact.

If the daily range of temperature is an impor-

[2] Mean 1866-70. "Tables of Differences," Smithsonian Contributions.

[3] Small variations in this case line are unimportant, on account of the small number of cases represented.

[4] Report of State Board of Health, Michigan, 1888, p. 25.

[5] "Tables of Differences of Mean Temperature," p. 156, Smithsonian Contributions to Knowledge.

tant element in the production of intermittent fever, why should a change of climate develop the fever? Dr. Baker says: "It is a common observation that persons who travel northward or to a colder climate, where ague is comparatively rare, not infrequently have one or more paroxysms of chills and fever soon after the change.[6] During the war I noticed this on a large scale after movements of troops from the South."[7] Dr. Baker's explanation is not a reasonable one for me, for the reason that the temperature changes are perhaps less in the Northern than in the Southern home. In Michigan, for example, Dr. H. T. Calkins, one of the oldest practitioners in the northern part of the State, and a close observer, tells me that at Petoskey they never have malarial fever, *except in persons recently from a malarious country*. I am told the same of Mackinac. If this is true, it is a most important fact, and my own observation leads me to accept it. An individual going from Southern to Northern Michigan will, unless specially protected, generally within from four to ten days show clear indications of malarial poisoning. The first symptom is not a chill, nor a fever, but a copper-colored skin. The purer air causes the blood at first to flow more freely through the cutaneous capillaries, which, showing through the jaundiced skin, misleads the victim to report excess of tan. A few days later and a chill occurs, followed by a fever, and if left to itself, the disease progresses until it assumes a typhoid type. For the production of this condition I am confident that it is not necessary to bring in a typhoid fever germ. The explanation of these phenomena which I have adopted, I think was first suggested by Dr. Bowditch, of Boston, though not in this exact form. The malarial poison, whether chemical or germ, acts upon the liver in such a manner as to slow the elimination of effete matter. The elimination of effete matter is accomplished through the liver, skin, kidneys and lungs. In the state of health these organs assist each other, keeping a normal balance. If an individual goes from a warm to a cold climate, the elimination through the skin and lungs is decreased. If the liver and kidneys are healthy, no harm results. If, however, as in the case of the malarial person, the liver is sluggish, it will be unable to do the work devolving upon it, and the system is poisoned with the non-eliminated effete matter. This irritates the brain and produces the chill and increase in temperature. This theory exactly agrees with my clinical experience. Antipyretics have little effect upon the disease until the liver is cleared. Catharsis is not the essential factor, because compound cathartic pills and other like drugs may be given until the patient is exhausted, without any material benefit. On the other hand, if the case is seen early, calomel may be given in so small doses as to effectually clear the skin, without producing catharsis.

If a person residing in a malarious country, not being sick at the time, takes a thorough mercurial treatment before going to the North or to the sea shore, he will probably escape the usual sickness. While I would emphasize the importance of extra care as to clothing in making this change of climate, I regard the preparatory treatment as the more important. In fact, a person

TABLES FROM WHICH THE ACCOMPANYING DIAGRAMS WERE DRAWN.

No. 13.—Average Temperature, 14 Stations. Average Daily Range Temperature. Intermittent Fever, U. S. A.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Av.
Average Temperature	39.12	14.13	48.18	57.28	66.17	73.59	77.99	76.55	70.85	59.29	48.99	40.86	61.19
Daily Range Temperature	9.70	11.51	12.20	12.75	14.39	13.75	13.60	13.62	14.04	13.45	11.69	9.77	12.58
Sickness	19.4	19.6	20.4	23.6	24.5	30.6	25.6	32.	38.5	36.8	28.1	19.3	26.5

No. 14.—Michigan Day and Night Ozone. Relative and Absolute Humidity. Average Temp. Per cent. Int. Fever.

Day Ozone	3.33	3.46	3.44	3.21	3.09	2.88	2.69	2.88	2.74	2.78	3.01	3.18	3.06
Night Ozone	3.91	4.04	4.07	3.56	3.31	2.98	2.55	2.49	2.54	2.99	3.35	3.55	3.23
Relative Humidity	.81	.80	.77	.69	.68	.72	.72	.74	.75	.76	.79	.82	.75
Absolute Humidity	1.38	1.51	1.81	2.75	3.91	5.27	6.07	5.84	4.98	3.71	2.30	1.73	3.44
Average Temperature	20.56	23.62	29.80	44.33	56.08	65.10	70.52	68.14	61.67	50.83	36.08	26.60	46.11
Per Cent. Fever	58.4	60.8	62.75	70.4	75.8	77.8	79.5	79.0	79.0	76.1	67.0	58.8	70.4

No. 15.—Deaths New York State from Intermittent Fever. Average Temperature and Daily Range. Mohawk.

Average Temperature	20.87	22.51	28.78	42.33	54.89	64.59	69.64	67.73	54.77	47.94	34.06	23.12	44.27
Daily Range Temperature	7.21	9.27	11.06	13.72	15.72	18.04	17.00	15.85	14.89	11.03	6.49	6.28	12.21
Deaths	176.	193.	196.	225.	208.	191.	204.	258.	325.	297.	247.	216.	228.

No. 16.—India. Av. Maximum and Minimum and Mean Temperature. Intermittent Fever Native Troops.

Av. Max. Temp.	76.0	83.0	88.5	98.2	97.3	95.4	87.1	86.4	88.1	87.0	82.1	77.4	
Av. Min. Temp.	41.2	39.8	50.1	56.4	61.1	63.0	66.9	66.6	65.5	59.2	50.5	43.8	
Mean Temp	58.6	61.4	69.3	77.3	79.2	79.2	77.0	76.5	76.8	73.1	66.3	60.6	71.3
Sickness	163.	156.	178.	181.	198.	297.	307.	517.	537.	535.	557.	244.	324.

No. 17.—Average Temp. Camp McDowell. Daily Range Temp. and Intermittent Fever Ft. Apache, Arizona.

Average Temperature	50.36	53.95	59.04	69.69	78.89	88.60	92.42	89.58	83.83	73.22	60.90	52.49	71.08
Daily Range Temperature	30.56	30.30	30.86	35.94	38.94	41.34	33.84	28.96	35.64	32.64	35.22	30.70	33.74
Sickness	4.62	9.02	14.77	14.89	11.62	17.06	25.59	56.10	52.12	40.73	26.69	7.70	23.41

[6] JOUR. AMER. MED. ASS'N, Vol. xi, p. 653.
[7] Loc. cit., footnote.

whose liver is active will need less clothing for comfort than if his liver is sluggish.[8] In some cases, it is necessary to continue small doses of the chloride or iodide for some time.

The usefulness of this treatment is generally known, and is admitted by Dr. Baker in conclusion seven. If, however, his theory is correct, any cathartic, decreasing the tension of the portal circulation, or heavy clothing would prevent the outbreak, as well or better.

How does a mercurial decrease the temperature? It certainly has an antipyretic influence upon a malarial fever or upon diphtheria, but it does not have any appreciable effect upon a healthy person. This effect is certainly not derived through increased heat elimination, and it must therefore be by reducing the production of heat. Since, however, it does not materially check the production of heat in a healthy person, this result must be the incidental result of the removal or neutralization of the irritating cause of the fever. If this is true the fever following a chill is not the result of non-eliminated heat being stored up, as Dr. Baker suggests. Further, if I am not mistaken, the experiments of Osler show that in intermittent fever there is increased heat production.

With regard to conclusions three and four it might be asked, how does Dr. Baker's theory explain the *cause* of delay in reaction from cold. The healthy person would react promptly; therefore this delay in reaction shows that neither the chill nor the fever represent the beginning of the disease. The mechanical effect of a chill may be a local congestion, but as soon as the cause is removed the system reacts and the only results are local primarily. If the intermittent fever is the result purely of the daily range of atmospheric temperature, since the body is daily exposed to the cold, the chill and fever would always be of the remittent type.

I am surprised to see conclusion six repeated. "In our climate those measures, such as drainage, which enable the soil to retain warmth during the night and thus reduce the daily range of temperature immediately over such soil, tend to decrease intermittent fever among residents thereon." Does drainage enable the soil to retain warmth? Almost any boy will tell you that sand in the sun is hot, but as soon as a shadow is thrown upon it it cools off. The rich black loam heats slowly and cools slowly. Dry climates are those most subject to great range of temperature. In northwestern Texas the traveler may suffer extremely from the heat by day, but at night he covers himself with a thick blanket and places a little water in a saucer from which he skims a thin layer of ice in the morning. In India we have already seen that there is the greatest range of daily temperature when the ground is dry. Dr. Chas. Denison took the records of 25 dry and 25 moist climates and obtained therefrom the mean daily range of temperature for four classes of places as follows:[9]

1.	Extreme dryness, 36.51°	Fahr.
2.	Moderate dryness, 20.63°	
3.	Moderate moisture, 17.09°	Fahr.
4.	Extreme moisture, 13.61°	

On the contrary, then, drainage increases the daily range of temperature, and so if Dr. Baker's theory is correct it ought to favor the production of intermittent fever.

On page 417 of the Report of the Michigan State Board of Health for 1880, I found the following report from one of the correspondents of the board. "During the first half of the month (August) the moisture in the air was as abundant *and the temperature so uniformly high*, that unhusked corn in the shock, also corn in the corn cribs began to grow vigorously. During the third week of the month malarial diseases rapidly arose, which I think was partially due to the *prolonged uniform heat* and moisture. There was also one half less azone during the first half than during the last half of the month." This report is for Washington, in the southeast part of the State. From the same report, pages 317, 321, 326, 344, and 349, I get the mean temperature, absolute humidity, day and night ozone at Washington, and mean daily range of temperature at Detroit as follows (having no like data for Washington).

TABLE 18.

MONTH.	TEMPERATURE.		ABSOLUTE HUMIDITY.	OZONE.	
	MEAN.	DAILY RANGE.		NIGHT.	DAY.
June	64.21	18.43	5.21	2.42	2.22
July	72.47	18.84	6.31	1.21	2.06
August	68.54	18.74	5.17	0.85	1.65
Annual Mean	46.44	16.60	4.01*	3.07	2.97

It will be noticed that during these three months of summer the thermometer kept constantly within the range we have before found the most favorable for the production of malaria. While the daily range of temperature was higher than the average it was not as great as in May (22.48°).

A monograph by Dr. G. H. Wilson, of Meriden, Conn., member of the State Board of Health, published several years ago, gives an account of the progress in a definite direction from southwest to northeast, of intermittent fever. This seems to be strong evidence that the disease is due to a specific poison or germ.

From clinical evidence and meteorological investigations therefore it appears:

1. That intermittent fever is due to a specific germ.

[8] I ordinarily prescribe two cathartic doses of calomel combined with ipecac, the first to be taken a week before starting, and the second three days after.

[9] Transactions of Ninth International Medical Congress, Vol. v, p. 40.

* Grains of vapor in cubic foot of air. Average for 9 months. No report for January, February and March.

2. That the development of this germ, and hence the production of the fever is favored by a most atmosphere ranging in temperature between 60° and 85° F.

3. That the germ is taken into the body by inhalation or through drinking water.

4. That the fever is the result of irritation of the heat producing centre of the cerebrum. This irritation is perhaps partially due to the direct effect of the specific germ, but it is also due to the retention in the circulation of effete matter.

5. That under favorable conditions the true malarial fever may assume the character of a genuine typhoid fever, without the agency of a typhoid germ.

6. Exposure to an average temperature below that to which the person is accustomed, may develop a latent malarial condition, but exposure to cold does not produce the disease.

7. There being no definite and constant relationship between intermittent fever and the daily range of atmospheric temperature, we must conclude that daily variations in atmospheric temperature are not essential for the production of the disease.

Ozone is a potent germicide. It seems to the writer that its influence upon the disease is incidental and negative. When ozone is abundant it attacks the germs in the air and deprives them of their virulency. There is less ozone over swamps than in the open fields; less in the winds filtered through the underbrush, than in the same winds before reaching the woods. There is more malaria on the windward than on the lee side of a swamp. Conditions favorable to the growth of malaria are therefore destructive of ozone.

That the presence of ozone in the atmosphere does not greatly increase the power of the human organism to withstand the effects of the germ, I am led to think from the fact that a person going from a malarious region, where the amount of ozone is small, to a colder region, where the amount of ozone is greater, is almost sure to have strong signs of malarial poisoning soon after arrival.[10]

The occurrence of the fever at high altitudes and in cold weather are sometimes mentioned as arguments against the paludal theory of the etiology of the disease. Let me simply hint at an explanation. One day after clambering up the crags on the coast north of Larne, Ireland, I was surprised to find myself in a bog while still close to the edge of the high cliff. Again: the upper plateau of Mackinac Island is 295 feet above the water's edge, and yet there one may find marshy ground.

The heating of the house in winter draws air from the cellar. The air of the house is not so rich in oxygen as that out of doors. One may therefore find in the house in winter the necessary heat, moisture, and decaying vegetation —all the atmospheric conditions known to be essential.

[10] Dr. Baker calls attention to the fact that persons exposed to night air over low lands are more liable to the disease than if they are exposed only through the day. It is suggestive that, according to Table 14, in Michigan, the months showing the most intermittent fever also show less night than day ozone, but generally there is more night than day ozone. Does light have any influence on the growth of the germ?

MEDICAL PROGRESS.

MODE OF ENTRY OF THE TUBERCULAR POISON INTO THE BODY.—A very good summary of the various paths of infection adopted by the tubercle bacillus is given by C. BOLLINGER in the *Münchener Med. Wochenschrift*, 1890, No. 43. He considers that the frequency of infection through the skin has been under-estimated. Several cases have been recorded of direct inoculation by wounds received from broken spittoons, etc., by bites, after circumcision, by morphia syringes, and earrings. Eczema and impetigo increase the susceptibility of the skin. No case has as yet been attributed to vaccination, and it would appear that the tubercle bacilli are unable to live in the vaccine lymph. They also appear unable to pierce the pores of the skin as do some of the pyogenic organisms. The susceptibility of the mucous membrane is increased by inflammatory processes, such as otitis, rhinitis, conjunctivitis, pharyngitis, etc.; from thence the poison travels to the submaxillary glands and those of the neck, and generally causes local tuberculosis of the glands. The chief point of infection is of course the lungs. Local predisposition is best exhibited by apices which have been before diseased, but have undergone a healing process. The movement is deficient both in expiration and inspiration, and the liability to reinfection is increased by anæmia, irritants (such as coal and metallic dust), constitutional influences, such as diabetes, disturbances of digestion, and unhealthy surroundings. The poison may pass through the lungs and attack the bronchial glands, under which circumstances the disease may be very insidious. The predisposition of the lungs again exhibits itself in metastasis; not every tubercular disease of these organs is due to inhalation of the bacilli or their spores. As regards primary tuberculosis of the testicles, joints, and bones, Bollinger considers that a latent hæmatogenic infection must be understood, which leaves as little trace of its point of entry as does a primary septic endocarditis or an osteomyelitis. Tubercular disease of the larynx depends upon an autoinfection through the sputum. The rarity of this disease in children is explained by the infrequency of pulmonary cavities in the rapid forms of phthisis.

Primary tuberculosis of the intestine generally, combined with an affection of the mesenteric and retroperitoneal glands, is usually occasioned by means of vitiated food and contaminated feeding utensils. Secondary tuberculosis of the intestine depends upon an autoinfection. The tubercular poison passes through to the intestine unaltered by the juices of the stomach and attacks the Peyer's patches and solitary follicles. Tuberculosis of the peritoneum, which is three or four times as common in men as in women, can arise directly from ulcers of the intestine, from tubercular abdominal glands, or, especially in women, from the urogenital tract; further, through contagion from the lungs and pleura, and finally in the course of miliary tuberculosis, or from caseous bronchial glands. Primary tuberculosis of the peritoneum is rare (3 to 4 per cent. of all cases). As regards the infection from milk, this is, in Bollinger's opinion, undoubtedly due to the udder of the cow being affected with the disease. Infection through the milk of tuberculous women has not yet been proved. In tabular form the organs of the body are thus affected, beginning with those most frequently diseased: 1, lungs, 2, the lymphatic glands, 3, intestine, 4, serous membranes, 5, larynx, 6, spleen, 7, joints, 8, bones, 9, liver, 10, kidneys, 11, the genital tract, 12, the skin, 13, the brain and spinal cord, 14, muscles.—*The Lancet.*

EHRLICH'S TEST IN TYPHOID FEVER.—This test, which has been known for a number of years, has till recently been regarded by many rather as a medical curiosity than as of diagnostic value. DR. C. E. SIMON, of the Johns Hopkins Hospital, has recently shown that by carefully following the precise directions for its use, valuable information may be derived. The test consists of two solutions. 1. A saturated solution of sulphanilic acid in five-per-cent. hydrochloric acid, and 2. A five-per-cent. solution of sodium nitrate. These are to be mixed, just before use, in the proportion of 40 cc. of (1) to 1 cc. of (2). If this mixture be added to urine from a case of typhoid fever, the further addition of ammonia will produce a play of colors varying from an eosine rose to a deep garnet red. The best method of applying the test is to take a few centimetres of urine in a test-tube, adding an equal quantity of the sulphanilic acid mixture and shaking thoroughly; 1 cc. of ammonia is then run carefully down the side of the tube. At the junction of the two liquids there will be observed a ring of the characteristic color, which is produced in scarcely any other disease than typhoid fever. Dr. Simon's conclusions may be thus summarized: 1. The reaction may be obtained in typhoid fever from the fifth to the twenty-second day of the disease. 2. Its absence from the fifth to the ninth day indicates a very mild attack, save in children, although this rule is not an absolute one. 3. As it occurs previous to the appearance of the rash, it is a very useful aid in the diagnosis of typhoid fever.—*Therapeutic Gazette.*

CARDINAL POINTS IN BACTERIOLOGY.—*The Bacteriological World* says:

The words germ, bacteria, microbe schizomycetes are used in our present literature almost as synonymous terms, but microbe seems preferable to germ or bacteria, and schizomycetes is a better scientific term than either.

That these are unicellular, and assimilate nourishment, seemingly by absorption in the media in which they live, but they must transform (alter) the foods found proper, and yet unfit in nature, for their use and appropriation.

Bacteria living on dead matter encounter no living resistance, whilst those feeding on living tissues, or fluids in living tissues, meet the living cells of the body and have to combat them.

The diastases secreted by the various beings, whether highly organized, or unicellular and microscopic, have something in common as to their respective objects, and their properties of transforming matter.

The rôle of microbes in the world is complex and necessary, though some are injurious. They act as scavengers, return to the air and water the organizable elements abstracted daily by the vegetables of the globe, and indirectly by animals, and indispensable to life.

The bacteria that invade living organisms which happen to be fit for their nourishment and growth, are in a sense parasites just as much as the tapeworm is.

Spontaneous generation of living organisms, no matter how little, is a fallacy.

EFFECTS OF STRYCHNINE ON THE STOMACH.—The effect of nitrate of strychnine on the functional activity of the stomach has been recently made the subject of a careful research by Dr. Gamper, of St. Petersburg, who employed for the purpose of his experiments four healthy young hospital assistants. He found that strychnine increased the amount of gastric juice secreted, the general acidity, and the quantity of free acid in the secretion. It also hastened the absorption from the stomach, and strengthened the mechanical movements. Its effect, too, continued for some time after its administration had been stopped. Like many other Russian observers, Dr. Gamper seems to have been highly impressed by the value of strychnine in chronic alcoholism, declaring that it is the most effective of all drugs in such cases. The thesis contains a long list of references to the literature of stomach affections, published in six or seven languages during the last ten years.

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LONDON OFFICE, 57 AND 59 LUDGATE HILL.

SATURDAY, JUNE 13, 1891.

MEDICINE IN JAPAN.

The progressive spirit of the Japanese people—and particularly in the direction of medicine—has of late been abundantly shown to Mr. Ernest Hart, editor of our justly distinguished contemporary across the seas, the *British Medical Journal*.

Mr. Hart has been temporarily sojourning in the land of æstheticism and flowers, and he finds very much to praise, but little to criticise, and an opportunity for suggestion, which, if wisely cultivated, may prove no less meritorious and successful in Japan than has maintained under the sterner régimé of the Anglo-saxon.

In the way of gratification and commendation the observer found the emergence of the nation as a unit from a vast superstitious thralldom of a not greatly-distant past; the rapid acceptance and appreciation of Science in its wonderful onward march; the newer systems of government; the improved architecture; and the advanced provisions in learning.

Upon the latter condition, it may be said—the advanced provisions in general learning—the former manifestly and entirely depend. They are simply results, sequences, as, indeed, has been the case the world over.

The Imperial University, with departments of law, medicine, engineering, literature and science, each thoroughly equipped, and especially—as trite to a medical mind—the department of medicine with a faculty of no less than fifty-five professors and assistant professors, many of whom had attained distinction in foreign countries previous to taking upon themselves the responsibilities of leading onward the minds of their countrymen, very much impressed the Englishman.

A light criticism rested upon the system of training nurses; upon bathing facilities in hospitals; and upon the general care and comfort of hospital patients, all of which was offered in the most friendly spirit.

Then as to suggestions, which were in the line of a national medical association, and a beneficent organization possibly in connection therewith.

The value of such creations were exhibited by a clear statement of the organization, objects, method, and condition of the British Medical Association; together with a rehearsal of what the British Medical Association had accomplished, and the position, usefulness, benefits, and general scope of the *British Medical Journal*. Also the insurance feature which has been so well planned and carried out under the patronage of the Association during the past few years.

Then, with a candor equal to that taken while in the attitude of representative, critic and teacher, the English physician craved knowledge from his hearers, the strange new people—a knowledge which he all but admitted could only come from them, however high a degree of scientific skill, or prolonged a research might be required for its development.

Among the questions the profession in Japan is asked to decide are these, viz., the etiology and communicability of leprosy; the pathology and prevention of beri-beri; the influence of prolonged lactation upon the mother's health, upon infant mortality, and the rate of increase of population; the comparative immunity of the Japanese from scarlet fever, and the rarity of its spread; the nature of senile (?) gangrene so commonly found in middle age in Japan; the influence of the customary hot baths, particularly in infantile pneumonia; the apparent unimportance of hæmorrhages in typhoid fever; and the real place of massage as a therapeutic measure.

Many of these are very pertinent inquiries, and if the Japanese physicians, working under the encouragement offered by the royalty, and the stimulus which comes from the earnest watchfulness and waiting of a great race, can effect a reasona-

ble solution to any or all of them, then will the finer genius of the amalgamation of minds moulded, it may be, by irrelative race characteristics, become more patent.

Mr. Hart said in his address before the Medical Faculty of Tokio: "These are among the subjects on which we should be pleased to offer you the hospitality of our medical periodicals, and as to which we should be glad to receive communications at the meetings of our British Medical Association."

To this desire for a broader interchange of thought, to this wish for a closer union of scientific minds whose objects remain the same despite the vast distance which divides, and the different blood—commonly speaking—which gives its nourishment, the profession in America may earnestly unite.

It is really a little surprising that the Japanese mind has so quickly reacted to the impulse of learning, and is to-day so far advanced as to provoke not alone the plaudits of the more pretentious, but to create an appeal to her scientific possibilities; and although digging into the depths of recondite medicine is not at all confined by national margins, and should not be, yet inasmuch as the eyes of the medical world have pointed eagerly towards England, towards France, towards America, and lately towards Germany, it is not without the limits of probability that we may all yet look with like eagerness in the direction of promising Japan.

HIGHER MEDICAL EDUCATION.

The promotion of all efforts to advance and raise the standard of medical education is always a vital subject with those who have at heart the best interests of our profession. And it is with a sense of gratification that we note from year to year a visible improvement in the standard adopted and approved by the leading educators of our country.

An encouraging feature at this time is the effort that is being made to affiliate and more or less directly connect the best medical schools with established universities.

This works advantageously to all such institutions; it enables students in the academic course to so arrange their studies as to make the last two years in the latter count as the first two in their more strictly professional studies.

A young man who has pursued his studies in this manner has much the advantage of those who enter a medical college with an indifferent preparation. In fact the one represents the trained roadster, that easily skips along and reaches the goal in good condition. While his fellow, of equally good blood and breeding, that that has had little or no grooming, stumbles over the slightest obstacle, shies at imaginary hinderances, indifferently obeys the guiding lines, frets, sweats, and is a good deal blown at the wind up on the home stretch.

The following circular issued by the Illinois State Board of Health sets forth the situation of this subject, and very properly solicits the aid of practical teachers in making up a science course that can be adopted as a standard:

ILLINOIS STATE BOARD OF HEALTH, OFFICE OF THE SECRETARY.

SPRINGFIELD, June 6, 1891.

Dear Sir:—There is a demand, from medical teachers and young men that intend to study medicine, for a literary course preparatory to the study of medicine. This demand has been met by a few of the literary institutions in the United States, and it is hoped and believed that it will be much more generally met during the next two years. The following institutions now offer science courses for students that intend to study medicine, or that intend to teach or otherwise engage in biological work: 1. University of Wisconsin; 2. University of Pennsylvania; 3. Johns Hopkins University; 4. University of Notre Dame; 5. Yale University; 6. Cornell University; 7. Princeton University; 8. Lake Forest University; 9. Northwestern University; 10. West Virginia University; 11. University of Kansas.

As must be obvious, such a course should be based on biology, and should include thorough work in this science, as well as in osteology, comparative anatomy and chemistry, with English, French, German, some Latin, clay modeling, free-hand drawing, mineralogy, mathematics through trigonometry, mechanics, logic, general and pharmaceutical botany, and (in the last year) psychology.

It is of course understood that botany, being a branch of biology, should have a prominent place in the course.

The catalogues of the universities mentioned contain the list of studies offered in their science courses.

Such a course should extend over four years. This will involve no loss nor waste of time to the student. The Illinois State Board of Health now requires that students of medicine matriculating in the autumn of 1891 or thereafter must study medicine four years and must attend three courses of lectures,—no two in the same twelve-month, in order to obtain a license to practice in Illinois. This rule will apply also in some other States. The Illinois State Board will, however, recognize a thorough course in science, such as indicated above, as equivalent to two years' study and one course of lectures, thus enabling the student to enter the second class in the medical college. This makes the full time of study six years in the literary and medical schools, or two years less than is required of the student pursuing a strictly classical course. Not only will time be thus saved, but the science student will be much better prepared to enter the second course of the medical school than will the classical student to enter the first year.

The Illinois State Board wishes to make up a science course that can be recommended to any college wishing

to adopt such a course, and having but little time to study the subject. I desire to enlist your aid and have your advice in the matter so as to make the course as practical and as beneficial as possible. Will your faculty, therefore, make out such a course as it thinks best for the purpose, and send it to the Secretary of the Board?

The demand from medical teachers and from students of medicine having been met by some universities, must be met by all that would continue to hold a high rank as educators of young men for the work of life. Such a course is also the best preparation for the study of veterinary science. Respectfully,

JOHN H. RAUCH, M.D., Sec'y.

SOCIETY PROCEEDINGS.

New York Academy of Medicine.

SECTION ON ORTHOPÆDIC SURGERY.

Stated Meeting, May 15, 1891.

SAMUEL KETCH, M.D., CHAIRMAN.

DR. T. HALSTED MYERS presented a case of marked rickets, called congenital on the mother's positive assertion that the greatly enlarged epiphyses of the tibiæ, femora, and radii were present when she first examined the child a few days after birth. The sternum at that time, she noted, also was abnormally prominent. She had been in very good health all through gestation, and the father was a healthy man. No specific history could be obtained.

At present, the child is six years of age, and presents all the deformities of rickets in a marked degree, except that the head is well shaped, and there is a marked increase of the normal dorsal curve of the spine, rather than the dorso-lumbar kyphosis usually found in these cases. An unusual degree of permanent knee and hip flexion also exists, and the patient assumes, when resting, the hand-to-knee position of Pott's disease. The epiphyseal tenderness present seemed to indicate an active stage of the disease. After being nursed nine months, the child had a mixed diet, not especially starchy, nor lacking in animal fats.

THE CHAIRMAN thought it not improbable that the spinal symptoms were the result of an acute lesion occurring coincidently with this diathesis. In cases of simple kyphosis which he had examined, one of the points in the differential diagnosis had been the absence of psoas contraction, and in most of these cases the curve, unlike this one, disappeared when the patient was in the prone position.

Dr. JOHN RIDLON exhibited the photograph of a patient, nineteen years old, who had had exactly this position all his life. There were additional curves in both the tibiæ and femora, which had developed gradually during his growth. It was worthy of note that psoas contraction was also present in his case.

DR. NEWTON M. SHAFFER had seen psoas contraction in these cases of rachitis. The case just presented was not, in his opinion, one of tubercular disease of the spine, but a sensitive condition of the cancellous structure in the bodies of the vertebræ which simulates Pott's disease. He had never seen a case which he could consider one of congenital rachitis, and he was inclined to look upon this one as an instance of rachitis acquired at a very early age. It was not uncommon to find in rachitic patients a condition of the muscles somewhat resembling that found in tubercular joints. He was reminded of a case which he had seen in St. Luke's Hospital, in which there was a very sensitive joint, associated with muscular symptoms which might suggest hip-joint disease, but these were simply due to hyperæmia of the epiphysis occurring in a rachitic subject, and in due time, with proper attention to nutrition, these symptoms disappeared.

Dr. RIDLON said that he had expected to present a patient illustrating certain peculiar conditions found in persons who had the caisson disease. His patient had been working in compressed air for sixteen years, and during the past year, had had forty or more attacks of the cramps which are peculiar to this disease. Associated with these were stiffness, gradual shortening, and outward rotation of the right lower extremity, with a direct upward dislocation of the hip for a distance of three-quarters of an inch. This man had informed him that he knew of a number of others who had been working in compressed air, who had paralysis with shortening of the limb.

THE CHAIRMAN said that he had seen a man, 40 years of age, who had been a caisson worker and diver, and who presented an affection of both hips. There was very little motion except in abduction. There was no history of rheumatism, or other constitutional disorder.

DR. SHAFFER had recently seen at the Orthopedic Dispensary a caisson worker, who presented bilateral hip symptoms, and who was scarcely able to walk. In this case, the symptoms were those of a pronounced rheumatic type, and the changes were apparently due to rheumatic arthritis.

DR. RIDLON also exhibited photographs of the latest modification of Grattan's osteoclast, and of some of the cases which this surgeon had treated by means of the instrument. He now used it for forcibly correcting club-feet, and in the opinion of the speaker, it was the handiest and most efficient contrivance of its class that he had seen.

NON-UNION AFTER OSTEOTOMY IN A CASE OF SEVERE RACHITIS.

THE CHAIRMAN presented a little girl with a very exaggerated form of rickets, whose symptoms indicated that the disease was still active.

The chief point of interest was the fact that about three years before, a skillful surgeon had performed osteotomy upon her for the correction of a severe form of bow-legs, and this had resulted in non-union. This case showed the folly of operating in the presence of such a virulent form of rachitis. The treatment in his hands had consisted in the application of a coaptation splint, and of a perineal crutch, which by means of a snap joint allowed motion at the knee, but prevented dangerous traumatism, and favored locomotion. The idea of the apparatus was to favor locomotion rather than to attempt to secure union.

DR. ROYAL WHITMAN doubted if this treatment would lead to union of the fragments, for the end of the bones in such cases become extremely hard, and usually require to be removed before union can be secured.

DR. R. H. SAYRE thought the non-union in this case might have resulted from the fact that the deformity was so great, that in order to correct it, a considerable interval must have been left between the ends of the bone after the osteotomy.

(*To be concluded.*)

DOMESTIC CORRESPONDENCE.

LETTER FROM BALTIMORE.

Judged by numbers, the late session of the medical schools of this city was a great success. The following was the attendance and the number of graduates:

SCHOOLS.	MEDICAL MATRICULATES.	MEDICAL GRADUATES.
	About.	
College of Physicians and Surgeons	358	110
University of Maryland	259	88
Baltimore Medical College	125	51
Baltimore University	88	32
Woman's Medical College	22	6
Total	852	287

The attendance has been equally good at the dental schools. The Baltimore College of Dental Surgery having about 226 students and 77 graduates, and the Dental Department of the University of Maryland about 163 and 64 respectively. The Maryland College of Pharmacy had about 125 students and 29 graduates. So that the total number of students and graduates in medical and allied schools in this medical centre (not including the biological and preliminary medical at the Johns Hopkins University and the post-graduate at the Johns Hopkins Hospital) is about 1,366 of the former and 457 of the latter, figures the size of which I suppose few outsiders would suspect.

The Maryland College of Pharmacy has just celebrated the semi-centennial anniversary of its foundation. Its lectures were at first delivered at the University of Maryland, which set apart quarters for its accommodation. Besides the commencement and commemoration meeting of the college, the exercises included a magnificent reception with music and lunch at the college building, and a banquet at night. There were visitors from New York, Philadelphia and other cities. The college is now in a very prosperous condition, has a large new building, well equipped, and has a high reputation for honest and faithful work in training young men for the pharmaceutical profession.

The College of Physicians and Surgeons introduced a novelty at their commencement this year, which has elicited criticisms both favorable and the reverse. It was the adoption for the graduates of the English cap and gown. The effect was very pretty, and the manifest advantages of having a cheap and uniform attire cannot be questioned. The college furnished the suits, charging a nominal sum for their use by the students. There does not appear to have been any difficulty or opposition to the adoption of the innovation. An attempt was made last winter to introduce the custom at the Hopkins, but it met with little favor, only President Gilman appearing in cap and gown at the anniversary exercises February 22.

Miss Mary Garrett has given another evidence of her liberality and interest in the higher education of her sex by offering to contribute an additional $10,000 to the endowment fund of the proposed Johns Hopkins Medical School. In a letter dated April 27, addressed to President Dobbin, of the Board of Trustees, she makes this offer payable October 1, 1892, subject to the following conditions: That by February 1, 1892, the trustees raise the remainder of the sum necessary to bring the endowment up to $500,000—the minimum limit proposed by the trustees—and further that the trustees shall give notice in February, 1892, that they will open the school in October, 1892, and shall actually open it then. The committee of ladies organized to raise $100,000, having accomplished their object, has disbanded. The following represents the present status of this endowment: The entire sum raised by the committees of ladies of Baltimore and elsewhere and paid over to the trustees May 1 was $111,000; there were already in hand additional funds sufficient to raise this amount to $178,780. In order to avail of Miss Garrett's offer, it is therefore necessary to raise $221,219. The trustees have accepted her offer and have resolved to endeavor to raise the balance required. There is no doubt that her interest is so great that she will extend the required time should it be necessary to do so. Of the $111,300 raised by the ladies, Baltimore contributed $68,882; the next largest contribution Boston's $20,231, and Philadelphia's $8,075.

Dr. George F. H. Nuttall, of San Francisco, a graduate of the University of Göttingen, has been appointed to the vacancy in the bacteriological and hygienic department at the Johns Hopkins Hospital, occasioned by the acceptance by Dr. Alexander C. Abbott of the directorship of the new Hygienic Institute of the University of Pennsylvania.

The death of Dr. John Frederick May, of Washington, recalls the fact that in early life he held the Chair of Surgery in the Trustees' Faculty of the University of Maryland, at the time when that school was split into two schools (1837-39).

The Sisters of Mercy are erecting a four-story addition to the City Hospital (connected with the College of Physicians and Surgeons), for the use of their colored patients, at a cost of $20,000.

Dr. James Carey Thomas, one of the Trustees of the Johns Hopkins University and Hospital, has gone to Europe, and one of the chief objects of his journey is stated to be to examine into the structure and architecture of European medical schools, with the purpose of utilizing the information in the construction of the Hopkins school.

The use of the Koch lymph is gradually falling into desuetude here as elsewhere. There never was much gush over it here, and its use at the Hopkins was confined to lupus and incipient cases of pulmonary tuberculosis. A judicious skepticism and timidity characterized the attitude of our physicians towards it from the start, and consequently we have had no deaths or bad results to report. The results as a whole have been disappointing, and although a few cases are still under treatment, the probabilities are that ere long it will become a matter of the past.

The death of Judge George W. Dobbin, at his country seat near Baltimore, on the 28th of May, at the age of 81, has removed from our midst one of our most distinguished and public-spirited citizens,and one who has been very active in the cause of education. Among the positions he held at the time of his death were Regent of the University of Maryland and Dean of its Law School, Trustee of the Peabody Institute and of the Johns Hopkins University and Hospital. From 1867 to 1882 he held office as Judge of the Supreme Bench of Baltimore, and for many years had been President of the Board of Trustees of the Johns Hopkins University. He was a most courteous gentleman with a cultivated mind and scientific tastes, and he enjoyed the respect, veneration and love of the entire community.

Another death which is deeply felt among us is that of Dr. Richard Gundry, the able specialist on insanity and superintendent of our State insane asylum, which took place from Bright's disease, on April 23. He was a native of England and 60 years old. He took charge of the asylum here in 1878, having held similar positions previously in Ohio. He was also Professor of Materia Medica and Mental Diseases in the College of Physicians and Surgeons of Baltimore since 1882. He was an enlightened physician, a man of scholarly attainments and high professional standing.

Professor Wm. C. Jarvis, of the University of New York, delivered the annual address before the Alumni Association of the University of Maryland, on April 1st, the subject being "Glimpses of Metropolitan Life."

A very successful concert in the interest of the University of Maryland Hospitals was held on April 10, under the auspices of the ladies connected with the Faculty of that institution. The object was to raise a fund for the increase of free beds, the hospitals being without endowment. Vladimir de Pachman, the pianist, was the chief attraction, and the entertainment was a great success, nearly $2,000 being cleared. The chief hospital of the University (formerly known as the Baltimore Infirmary) is one of the oldest college hospitals in the country, having been erected and opened in 1823, and being entirely under the control of the Faculty, and immediately adjoining the University building, it has by its clinical advantages enabled the institution to take a high rank among the medical schools of America.

The ninety-third annual meeting of the Medical and Chirurgical Faculty of Maryland was held April 28–30, under the Presidency of Professor Thos. A. Ashby, who delivered a stirring address on the need of better professional organization in Maryland. The paper of the meeting was undoubtedly that of Professor Wm. H. Welch, on the "Causation of Diphtheria." It has since been published in full in the *Medical News*, and should be read by every one who wishes the latest information on this disease from a master hand. In honoring Prof. Welch with the Presidency of the Society for the ensuing year, the Faculty has conferred still greater honor upon itself.

Dr. Randolph Winslow, Professor of Surgery in the Woman's Medical College here, has been elected to the chair of Anatomy and Clinical Surgery in the University of Maryland, in which he was formerly Demonstrator. Dr. I. K. Trimble has been elected Professor of Anatomy in the Woman's Medical College, vice Prof. Jay, transferred to the chair of Practice of Surgery. There are rumors of approaching changes in the *personnel* of the Johns Hopkins medical staff. It is announced that the organization of the "Southern Homœopathic Medical College" of this city has been perfected by the election of a Professor of Surgery (from Washington), and that the lectures will begin in October next in a rented building. When the Hopkins opens in 1892 we will have seven medical schools here!

It is to be hoped that the action of the American Medical College Association in Washington, in reaffirming by a large majority its determination to exact a preliminary Latin requirement, will settle that matter for the future with the medical schools of the country,and that those who voted for it and against reconsideration were sincere in their action. The only two schools here that did so were the University of Maryland and the Baltimore University. The former institution is making ready for the institution of the three-year graded course in the fall, by the institution of histological and chemical laboratories. It has not been announced yet who will take charge of the former.

I send you the following compositions on George Washington, which are veritable curiosities in the way of preliminary accomplishment, showing as well as anything could the need of some standard at least of requirements in those who undertake the study of medicine. They were handed in by two candidates for matriculation in one of our medical colleges, *one of whom had been a teacher of a public school.* I am glad to say both were rejected.

I. "George Washington When a boy could not tel a boy could not tel a lie he cut one of his fathers Chury trees with his hatchet when Father called him and aske who cut the tree he said I cut it with my hatchet he became a man of zeal and industry was belove by all who

knew him he fought many battels aud by his cunning defeated Nepoleon he maried became the first president of the United states lived to a good old age. Oct 1st."

II. "jeneral Washinton was born in Virginia he was a truthfull Boy a good Chresttian a noble man in war & in peace he was the firist President of the United States of Amarica he was and considered the fathar of our country By his bravery whe are a free and independent people to think and act as whe think Best he was honest in all his ways."

I give them as near literally as I can. Yours truly,

E. F. C.

LETTER FROM PHILADELPHIA.

(FROM OUR OWN CORRESPONDENT.)

Although la grippe, influenza, dengue, horse distemper, "it," the great unknown, or by whatever name it may be known to future medical historians, has not favored Philadelphia this spring with such obvious marks of attention as it has some other cities of the Atlantic sea board, yet this may be explained by the fact that it committed such ravages a year ago that the susceptible material was largely exhausted. However, we have had sufficient number of these cases to supply clinicians with abundant material for study. In addition to typical cases of the disease, which are comparatively few this year, there are large numbers of abortive cases, showing the poison is still with us, and is responsible for a large amount of general ill health and lowered vitality in the community. In one respect this epidemic differs from epidemics of dengue. The late Prof. Samuel H. Dickson, formerly of Charleston, Va., noticed the fact that dengue was self-protective and that persons who had an attack would not be liable to suffer at the next appearance of the epidemic. Quite a number of persons who had influenza last year have it again this year although, it is true, as the rule, in a milder form. In the treatment, quinine seems of little use and only adds to the patient's discomfort. Antipyrin, acetanilide, sulphonal and other agents of the aromatic group, can only be used cautiously, if at all, on account of the tendency to lower the action of the heart, but where the pyrexia is marked, salicylic acid or phenacetin in small doses (gr. iii, v) given every hour or two, is decidedly useful. Small doses of Dover's powder, with a little camphor, or of the compound morphine powder for restlessness; ice to the head if there is headache; small pieces of ice swallowed when there is nausea, and a dose of calomel with jalap or rhubarb, for constipation, are often followed by marked relief. In addition to this, a good elixir of coca (Lorini) or the well-known Mariani wine or caffeine citrate will be generally needed to support the weak heart, and nervous system. During convalescence, ignatia or nux vomica should be an appropriate remedy; but patients seem very susceptible to strychnine and some complain of cramps and muscular pains. The hypophosphites with iron, form a good tonic, but the best restorative after all is a change of air to the mountains or seashore, with nourishing food given only in moderate quantity and at regular times. The above brief outline of treatment has proved uniformly successful in the experience of the writer, who has had no deaths and few complications. In delicate nervous cases it contributes very much to their comfort to dispense with linen sheets and to substitute woolen ones, or just put the patients between blankets.

A movement is on foot to increase the endowment of the Medical Department of the University of Pennsylvania. The Trustees have accepted the offer of the Faculty to contribute $10,000, and of the Provost, Dr. Pepper, to give $50,000, towards a fund for additional endowment of Medical Department of $250,000, and will lengthen the term to four years when the fund is made up; it is expected to be raised within five years. It is possible that this fund will be raised and that the University will be placed in a position to extend the course in medicine so as not to fall behind in the race with Harvard and Johns Hopkins. The death of Prof. Joseph Leidy was a severe loss to the school and to the community; but the Board of Trustees seem in no hurry to fill the vacancy. The prospects at present are that the Chair will be divided into surgical anatomy, for which Dr. John B. Deaver is an eligible candidate; and historical anatomy which may be offered to Dr. Harrison Allen, who was formerly Professor of Physiology in this institution and resigned.

At a meeting held June 2, 1891, the following appointments were made: Dr. George A. Piersoll was elected to the Chair of Human Anatomy, Dr. Harrison Allen to that of Comparative Anatomy, J. P. Grover Griffith, Prof. of Diseases of Children, in place of H. A. Hare, resigned. Dr. Edward Martin was elected Professor of Genito-Urinary Diseases, John D. Deaver was made Assistant Professor of Applied Anatomy.

The Jefferson Medical College has lost within a short time two of its most distinguished professors: Prof. Bartholow and J. M. DaCosta. The place of the former was promptly filled by the election of Dr. H. A. Hare; that of the latter is still vacant. Drs. J. C. Wilson, Frederick P. Henry, both of Philadelphia, and Jas. T. Whittaker of Cincinnati and several others, have been discussed as candidates, but the issue is uncertain. The Medico-Chirurgical College also has a vacant chair, Dr. Wm. T. Waugh, Prof. of Practice of Medicine, having resigned. It is understood that he is not without hope of a transfer to the identical chair at Jefferson.

There is a prospect that the state of Pennsylvania will give the Medico Chirurgical Hospital $70,000, to enlarge its buildings; the legislature has passed the bill which only awaits the signature of the Governor.

The medical societies of this city keep up their scientific work manfully in spite of the influenza and unfavorable weather. At the College of Physicians' meeting last week (June 3), Dr. Oscar H. Allis read a paper on Fracture of the Condyles of the Humerus treated by Extension—with a suit for malpractice and a verdict for the defendant, who was C. E. Kurts of Bellaire, Ohio. The last meeting of the County Medical Society was enlivened by a tilt between the operating gynæcologists, lead by Joseph Price, and the disciples of Apostoli, and his electrical treatment for uterine growths, of which Dr. Betton Massey is an able exponent. The conflict has been going on for several years but the ardor of the contestants

seems to be increasing rather than abating; to outsiders it is quite entertaining.

The Society of the State of Pennsylvania, during the past week, (June 2, 3, 4 and 5,) has been holding its forty-first annual session in the city of Reading, about sixty miles from Philadelphia, in the coal and iron region. It was not as largely attended as usual. The principal addresses were that of the President, Dr. Allen Craig, of Columbia; that on Medicine, J. Chris. Lange, of Allegheny City; on Mental Disorders, Samuel Ayers, of Pittsburg; on Ophthalmology by J. A. Lippincott, of Pittsburg, and on Obstetrics by J. Milton Duff, also of Pittsburg.

LETTER FROM NEW YORK.

(FROM OUR OWN CORRESPONDENT.)

The recent annual meeting (its seventh) of the Fifth District Branch of the New York State Medical Association was one of the best attended and most attractive that it has yet held, and its varied scientific proceedings were of a high order. The President, Dr. Stephen Smith, took for the subject of his address "Opiates in the Treatment of Acute Peritonitis," and it was devoted principally to a detailed and graphic account of the introduction into Bellevue Hospital, by the late Prof. Alonzo Clark, of the opium treatment of puerperal peritonitis. In the course of it he quoted the following passage from Dr. Clark's description of the outbreak of puerperal fever at Bellevue in the year 1851. "In three or four days seven cases were sent me from the lying-in wards. One was returned for error in diagnosis, and six put under treatment. Having proved that prudence was so much more conspicuous in any house physician than courage, another house officer, who combined them both, was selected to be in almost constant attendance. The instructions were in these words: 'I want you to narcotize those women within an inch of their lives.' He did it, and saved every one of them. This gentleman is now known over the whole land as a learned and distinguished surgeon. I feel called upon to give his name in this connection, that he may be a witness to the facts I state, and for the admiration with which his nerve and prudence impressed me." Dr. Smith said that notwithstanding this assertion Professor Clark failed to give the name of the surgeon referred to, but as he himself was then a resident student at Bellevue Hospital, and had charge of the six cases mentioned, he assumed that he was the person meant, and would take this occasion to witness to the facts recorded.

Up to this time mortality from puerperal fever at Bellevue had been appalling. Of 21 cases reported by Dr. Vache, the resident physician in 1840, 19, or nearly 90 per cent., proved fatal. This death rate, he said, was not unusual at that period, and the treatment was so mixed that it was difficult to draw any conclusions as to the efficiency, or rather inefficiency, of any one remedy. Still it was noticeable that in 17 of the 21 cases purgatives were actively employed, and that in twelve of these calomel and turpentine, two approved antiseptic agents, were administered freely. Up to the time when the opium treatment was adopted, as Professor Clark remarks, "peritonitis was a fearful word; a large proportion of those attacked by it died of it." Dr. Smith described in detail the treatment of the six cases placed under his charge by Professor Clark, who gave the most particular directions as to their care. At the outset each patient received 1 gr. of opium, or its equivalent, every hour for three doses. No effect being perceptible, the dose was increased to 2 grs. every hour, and continued for three doses. Failing with this dosage, the opium was increased to 3 grs. every hour. In short, he was especially charged not to be governed by the amount administered, but by the effects produced. In four cases the last-mentioned dose had the desired effect, and the opium was continued in that quantity. In one of the remaining cases it had to be increased to 4 grs. every hour, and in the other to *12* grs. every hour. This case proved an anomaly in tolerance of opiates. During twelve days the patient took the equivalent of 1,950 grs. of opium, and yet at no time was she so narcotized that she could not be roused by a touch on her wrist.

The recovery of these six consecutive cases naturally produced a profound impression, and as the cases were typical of those ordinarily occurring in Bellevue, and as opiates were the sole remedies employed, it was apparent that a new era in the treatment of this affection had begun. From this time forward opium continued for twenty years to be the chief remedy used in peritonitis in this hospital, and with satisfactory results. Opium in peritonitis came into use generally in the city of New York, and Professor Clark stated that in private practice the drug had been, perhaps, more curative than in the hospital. Happily, in these latter days, said Dr. Smith, antisepticism had greatly diminished the frequency of this formidable disease, especially the puerperal form, and in the meantime the remedial measures had been greatly increased. One of the most important results of recent investigations was the differentiation of the types of peritonitis caused by various agencies; and as there were many types of this affection, having their causation in various conditions (some being of microbic and others of non-microbic origin), it was as yet by no means certain where the line was to be drawn between the medical and surgical treatment. Whatever might be the possibilities of the art of surgery in the treatment of peritonitis, the question, he thought, might be pertinently asked, Has any remedy or combination of remedies in the hands of the physician given results equal to or better than opium, in a series of cases similar to those recorded in this paper? And if it be true that by employing opium, as recommended by Professor Clark, more than 50 per cent. of cases of peritonitis, as met with in practice, can be saved, should not this method of treatment be at once adopted in all cases, and the resources of surgery be applied when the manifestations of the disease indicate the necessity of its additional remedial measures?

An interesting discussion followed the address, but it was greatly regretted that Dr. A. Palmer Dudley, who had been announced to read a paper on "Cathartics in the Treatment of Acute Peritonitis," failed to make his

appearance. In the course of the discussion Dr. H. L. Carroll remarked that it was noticeable that the advocacy of saline cathartics in peritonitis came, as a rule, from the abdominal surgeons, such as Lawson Tait, for instance; and Dr. T. H. Manley, in referring to the discussion on the management of peritonitis in the Surgical Section of the American Medical Association, at its Newport meeting, spoke of the extreme opposition with which everything in the way of palliative treatment was met on that occasion. For his own part, he said, his experience had led him to regard the opening of the peritoneal cavity as a more serious matter than many seemed to regard it, and it was his belief that in the next ten years there would be fewer laparotomies performed for the relief of inflammatory conditions than had been the case during the past ten years. So much interest was manifested in the subject that, at the suggestion of Dr. Gouley, it was determined to request that at the general meeting of the State Association in October next, a discussion on the treatment of peritonitis, to be opened by a paper from the President, Dr. Smith, should be arranged for.

P. B. P.

(*To be concluded.*)

"M.D., Indianapolis."

To the Editor:—During the past few weeks a case was brought up in the courts of this city which induced very free remarks relative to the requisite standing of a man in the States to possess the title M.D. S. E. L. Smith, of this town, was registered as a licentiate of the Society of Apothecaries, which gave him rights to practice. But said Smith assumed the title of "M.D., Indianapolis," and he was summoned into court, fined the maximum fee of $100, with probably a quarter as much costs. Mr. Smith produced in court, during the trial, a document which gave him the title of "M.D., Indianapolis." It was said to have cost him $30. The medical man who prosecuted the case said that such a title could only be one of those foreign or American titles which could be got for a dollar or two.

It might be well for the young men of the States to know something of this "*$30 M.D., Indianapolis*" mill. We especially recommend it to the proud men in the city of that name. Mr. Smith was prosecuted and fined because he *assumed* the title of Medical Doctor (M.D.). The titles M.D., etc., said the prosecutor, enable the public to distinguish between qualified and unqualified practitioners. The assumption of a title would mislead the public. We are sorry to hear of the "bogus diploma" again, and thought it died a natural death in Philadelphia. It chagrins a medical man of the States to listen to the English courts ridicule our medical titles as being worth "a dollar or two." But we cannot help thinking that we deserve it to some extent. A student in England studies five years of about eight months a year to get his title. I know scores of medical men in the States who got their title in two winters' study of five months each. Certainly we cannot expect very high regard from a European court toward titles so easily acquired. It is an unconsoling argument to say that the American student is such a genius that he can learn as much in two winters as a European can in five years. There is but one way out of the matter, and that is to lengthen the course of study, and prosecute the bogus diploma man. Respectfully,

FRED BYRON ROBINSON.

Birmingham, Eng., May 26, 1891.

MISCELLANY.

AN INTERNATIONAL MEDICAL CONGRESS.—The managers of the National Prohibition Park, of Staten Island, invite representative medical men from all localities in the United States and Canada to meet in conference on the 15th and 16th of July next, in the great Auditorium Building of the Park. The chief object of the meeting is to be the comparison of views on the relationship of physiology and alcohol. Among the questions to be discussed will be the following:

What are the Hereditary Effects of Drunkenness?

Are there any Hereditary Effects that Follow Moderate Drinking?

To what Diseases are Inebriates More Especially Exposed?

Is Alcohol a Poison?

Is Alcohol in Any Sense a Food?

What are the Proper Uses of Alcohol as a Medicine?

Is there Danger of Producing the Drink Habit from the Prescribing of Alcoholic Medicines?

How Large a Percentage of Deaths May be Attributed, Directly or Indirectly, to the Use of Strong Drink?

Should Alcoholic Liquors ever be Used Except under the Direction of a Medical Adviser?

At this Conference, all views will be given an impartial hearing. No restraint will be placed upon the discussion save that of the time limit. Many well-known medical men have already signified their willingness to participate in such a conference. No harm, but much good, may come from this conference of views by leading physicians.

Official List of Changes in the Stations and Duties of Officers Serving in the Medical Department, U. S. Army, from May 30, 1891, to June 5, 1891.

Capt. Jefferson R. Kean, Asst. Surgeon, is granted leave of absence for one month, to take effect after the return of Major Robert H. White, Surgeon, to duty at Ft. Myer, Va. Par. 1, S. O. 123, A. G. O., May 29, 1891.

Major David L. Huntington, Surgeon, is granted leave of absence for three months, to take effect on or about June 15, 1891. With approval of the Secretary of War. Par. 2, S. O. 124, A. G. O., June 1, 1891.

Capt. Edward C. Carter, Asst. Surgeon, will proceed without delay to Ft. Canby, Washington, and report to the commanding officer for temporary duty, relieving Major John D. Hall, Surgeon, who will proceed to Ft. Sherman, for duty as Post Surgeon. Par. 1, S. O. 74, Dept. of the Columbia, May 22, 1891.

Capt. Henry P. Birmingham, Asst. Surgeon, extension of leave of absence on account of sickness granted in S. O. 108, May 12, 1891, from this office, is further extended to June 21, 1891, on surgeon's certificate of disability. By direction of the Secretary of War. Par. 6, S. O. 125, A. G. O., June 2, 1891.

Major John D. Hall, par. 1, S. O. 74, Dept. of the Columbia, May 22, 1891, transferring him from Ft. Canby Washington, to Ft. Sherman, Idaho, is confirmed. By direction of the Secretary of War. Par. 4, S. O. 126, A. G. O., June 3, 1891.

Capt. Van R. Hoff, Asst. Surgeon, is relieved from duty as a member of the board of medical officers to which he was assigned by par. 6, S. O. 78, April 7, 1891, from this office, and will, upon the completion of the duties assigned him by par. 6, S. O. 110, May 14, 1891, from this office, return to his proper station, Ft. Riley, Kan. By direction of the Secretary of War. Par. 11, S. O. 126, A. G. O., June 3, 1891.

Capt. Jefferson R. Kean, Asst. Surgeon, is assigned to temporary duty at Ft. Myer, Va., until the return of Major Robert H. White, Surgeon, to duty at that post. By direction of the Secretary of War. Par. 8, S. O. 122, A. G. O., May 28, 1891.

Official List of Changes in the Medical Corps of the U. S. Navy, for the Week Ending June 6, 1891.

Surgeon C. U. Gravatt, detached from Naval Hospital, Yokohama, Japan, and ordered home.

Surgeon Franklin Rogers, detached from special duty, Norfolk, Va., and to Yokohama Hospital.